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Photo by Gilliam Service.

An illicit whiskey still in the Carolina mountains

MOONSHINE LAND **[See page 68]**

Variable Atomic Weights*

With Some Reference to Geologic Time

By H. S. Shelton

THE present short article arises out of a discussion held at the Geological Society two years ago. In that discussion I expressed the opinion that the hypothesis of variable atomic weights was contrary to the general trend of chemical knowledge, and that it was not probable that the determination of the "lead" in a mineral would give a clue to the origin of that constituent. That point, a minor one in the discussion, I must present to my opponent, Mr. Holmes (see note at end of article). Whatever conclusions Prof. Soddy's theory of isotropes may eventually lead to, he and Prof. Richards seemed to have fully proved that the end product of uranium differs in atomic weight from that of thorium, and that both differ in atomic weight from ordinary lead. It will, however, be of some service to show in what way the hypothesis of variable atomic weights seems to me to be unsound, and how Prof. Soddy's theory of isotropes, whether or not it is true in all respects, is not open to the more obvious objections to the general theory. Moreover, as Prof. Soddy's recent address contains some remarks on geologic time, and apparently gives the weight of his authority to controversial views on a subject concerning which his opinion is hardly as valuable as is his study of isotropes, that aspect also calls for brief comment.

It will be convenient, first, to state the general objections to variable atomic weights. This idea, not, I believe, entirely unknown to chemical theory, postulated that the atoms of a chemical element, which chemical element was supposed to be in the mass in all respects homogeneous and inseparable, might vary in weight largely and symmetrically about a mean, the mean being the "atomic weight" of the element. This concept has always appeared to me, and also to others, as fundamentally unsound. In so describing it I am not merely thinking of obvious objections such as the possibility of separating the molecules of lighter mass by diffusion. I am raising a demurrer on principle against the idea that a hypothetical definite structural entity the atom, to which the properties of the element are referred, should vary in structure without also thereby varying in properties. Such a hypothesis appears self-contradictory. If the atoms be regarded as entities varying symmetrically about a mean, so must the properties referred to the atom.

At the time of the discussion at the Geological Society I had not seen Prof. Soddy's original paper, *Chemical News*, CVII, '97, and even on reading it subsequently I was not quite sure that his hypothesis was not open to these objections. I had, indeed, partially written a paper mentioning such matters as density and diffusion. Moreover, the original paper appeared to me to postulate far too absolute a degree of inseparability, more than was warranted either by valid theory or by fact. The last objection still holds, but recent developments have rendered the others inapplicable. Prof. Richard's discovery of the variation of the density and the constancy of the atomic volume, and Prof. Soddy's recent suggestion of the possible separability of isotropes by diffusion, show clearly that the isotropes are neither identical nor inseparable. That being so, the objections on grounds of general theory cease to apply. Criticism must become more detailed.

Indeed, Prof. Soddy's theory in its present form is not a theory of variable atomic weights. Stated briefly, it may be described as the assertion that there exist a number of different substances, each having a definite and distinct atomic weight and density, which substances are inseparable by chemical means. Even if we accept the statement at its face value, it is as well to emphasize that what is asserted is merely the existence of different bodies having certain identical properties. It is obvious, therefore, that the theory cannot be criticized merely on general principles, much in the same way as we cannot say on general principles whether or no different substances have the same density or melting-point.

The few criticisms that follow I shall confine entirely to the assertion of absolute chemical inseparability, leaving aside for the present that more tempting ground the wider ionic theory of matter. Prof. Soddy is far too dogmatic, more so than is warranted by the facts. Nor does he improve his case by the statement that "no one who has ever really tested this conclusion now doubts it, and after all they alone have a right to an opinion." It is not my intention to depreciate either the experimental ability of Prof. Soddy or the importance of the series of facts he has brought to light. But the claim that criticism of value can come neither from the general scientific public, nor even from competent chemists, is not one that can be granted to any group of investigators. The most he is entitled to state dogmatically is that when he performed certain experiments he obtained certain results, a statement which I for one am not disputing.

The facts as stated merely show that a certain series of experiments, mainly fractional crystallization, within the limits of experimental error, has failed to alter the relative proportion of certain isotropes. Accepting the statement fully it does not warrant the conclusion. That fractional crystallization fails to separate certain compounds may in itself mean no more than that nitrous oxide and carbon dioxide are non-separable by diffusion, or that certain mixtures of liquids are non-separable by fractional distillation. It merely proves the approximate identity of certain properties, and is not allowable to extrapolate unduly the range of properties that are identical. Needless to say I am not contending that the isotropes of lead are as different as nitrous oxide and carbon dioxide; the example is merely an illustration to show what the facts do and do not prove. Fractional crystallization has, in practice, proved the most reliable method of separating the compounds of nearly allied elements; it does not follow that it is the most efficacious when a new problem, a new degree of similarity, has to be dealt with. The rational conclusion from the facts is that the resemblances and differences between isotropes is a problem that requires fuller and more thorough investigation.

After the test case of ionium and thorium, few, I think, will doubt that isotropes are a real discovery. That more than one element could occupy what, on current theory, appeared to be a single place in the Periodic Table has long been familiar in the iron, palladium, and platinum groups. In these cases the elements showed a surprising dissimilarity of properties. In the lead and thorium isotropes we are faced with an equally surprising similarity. The discovery therefore calls for a variety of methods in the attempt to isolate the isotropes. It would be surprising indeed if there were no differential mass action (I am using this term in a sense somewhat different from the ordinary), in chemical processes, and if the distinction between chemical and physical processes were as absolute as Prof. Soddy appears to think. Moreover the somewhat uncertain character of spectroscopic evidence calls for caution before large inferences are based thereon. Instances when infinitesimal quantities show strong spectroscopic effects, and, on the other hand, when considerable quantities show none at all, are too well known for it to be possible to say outright whether a known spectrum refers to one element or to more than one.

These few criticisms imply no depreciation of the value of the discovery so admirably expounded by Prof. Soddy. But for the special circumstances in which the idea first came to my notice they would probably not have been written at all. There does, however, appear to me to be a certain theoretic interest in showing that the theory of variable atomic weights is untenable, and that Prof. Soddy's theory cannot be classified under this heading. There is also a very practical interest in pointing out that the next step is the careful study of the isotropes with a view to separating them, a step that should be taken without any strong preconceived idea regarding an absolute separation between chemical and physical properties. I purposely leave these conclusions in this vague general form, and have not overloaded the paper with specific suggestions concerning practical methods of separation in order not to distract attention from the paragraphs that follow.

So far I have been dealing with subjects concerning which Prof. Soddy is specially competent to speak. The subject of geologic time, however, is one which I can claim to have made my own. On this subject my criticisms of Prof. Soddy's remarks will consist in the main of the re-emphasis of conclusions the full reasons for which have been given in work previously published. Some of them may be of service to Prof. Soddy in that they will attach a greater relative importance to his estimates of the life periods of radio-active elements compared with those which some geologists think reasonable.

On no subject is it easier to publish a learned dissertation on some phase, or, now Boltwood has shown the way, to do a few more experiments on lead ratios, and thereupon assume the role of an expert and make "estimates" of geologic time of little greater value than those of previous generations. Lord Kelvin's three famous methods all in agreement should be a warning to rash successors of smaller ability and knowledge. I must, therefore, first strongly dissent from Prof. Soddy's statement that "the conclusion that lead was the ultimate product of the uranium series—has been the means, in the hands of skilful investigators, of ascertaining geologic ages with a degree of precision not hitherto possible."

This statement conveys a false impression if it is not entirely erroneous. It is not correct to state that geologic ages have been "ascertained" at all. There have been a few wild guesses, that is all. With regard to every method except that of lead ratios and pleochroic halos I have pointed this out so often and so thoroughly that further argument is superfluous. There has been no reply, and, so far as I am aware, no attempt to reply. And a very few words will suffice to show that the radio-active method, though undoubtedly a step in advance, cannot as yet be said to have "ascertained" any geologic ages whatever.

The first reason will appear to Prof. Soddy. He has been the means of showing that this method has been so uncertain as to seem to render very improbable the now established conclusion that an isotrope of lead is the end product of the thorium series. Let us take his own example of an average thorianite containing, say, 70 per cent of ThO_2 to 30 per cent U_3O_8. The amount of lead to be expected is about double that arising from the uranium. Analysis of thorium minerals failed to show its presence. The method is so inexact as, in this instance, to make an error of 100 per cent. With purer thorium minerals, which are not unobtainable, the error is greater still.

I am inclined to think that this proved error is small in proportion to the real error, indeed that the method is of value only in giving us some clue to the order of the result. In support of this statement the following elementary reasons, which can be amplified, extended, and generalized by going deeper into the subject, will suffice.

The method assumes that, for vast geologic ages, the minerals analyzed have remained unchanged in composition, that no material has diffused in from outside, that no material has shifted to surrounding strata, that the new elementary substance (lead) formed in the mineral has remained localized in the identical spot in which it has been formed. Although the theory of mineral formation and of the diffusion of minerals through strata is in a very rudimentary state, it is possible to say that the assumption is contrary to the entire trend of geological knowledge. Such a fixity of material, if it occurs at all, must be regarded as very exceptional. The chemical constitution of formations commonly changes in most weird and inexplicable ways. As an example, the well known phenomenon of pseudormorphs will serve us very well. Fossil forms are known, originally siliceous, now consisting of carbonate of lime. Others originally calcareous, now consist of iron pyrites. Moreover, there seems to be a general tendency for particles of like substances to segregate. The great degree of purity of minerals and the formation of flints are examples of this. This fact again gives an added improbability to the assumption that the lead formed subsequently to the mineral remains quantitatively localized where it is formed. Before making such an assumption we need very cogent evidence.

Of such evidence there is none at all. All we can say with certainty is that lead is known to be a product of uranium and that lead formed from uranium is present in uranium minerals, and that as a general rule, with many exceptions, the ratio lead to uranium increases with the geologic age of the mineral. Of quantitative permanence, absolute fixity of composition, there is no evidence whatever. The evidence as we know it agrees with our common experience of the semi-fluid nature of all solids if a sufficient time be allowed. Conditions under which it is allowable to assume absolute permanence of composition, the fixity of actual particles for hundreds of millions of years are very rare indeed, and are not found under the general conditions of the substances of which lead ratios are determined. On this ground alone the error may be not one hundred per cent, but many hundred per cent. Other sources of error that may be mentioned are: (1) the impossibility of knowing what proportion of the lead in the mineral is due to the uranium contained in it and what proportion to other sources, particularly original lead occluded in the mineral; (2) errors of analysis which are particularly large when the amount of lead is less than one per cent; (3) the difficulty of knowing exactly what is the geologic age of a

*From the *Chemical News*.

mineral, which is not so easy to determine as is that of a stratified rock.

From these few critical notes, which by no means exhaust the sources of error, there emerge two practical conclusions. The first is that dogmatism on the subject of geologic time on the part of those who have given some attention to radioactive minerals is entirely inadmissible, that the determinations are of value only in indicating the order of the time of geologic eras, and that the particular estimates may easily be wrong by many hundred per cent. No determination can be of much greater value which does not do finally and successfully what I have done for the present day and generation, coordinate the arguments drawn from many lines of investigation. At present to put forward actual figures in anything but the most tentative manner is entirely inadmissible and savors of pseudo-science. The second conclusion is that those who like Prof. Soddy are investigating the properties of radioactive elements need not at present, in their estimate of the life periods of the various disintegration products, take serious account of apparently contradictory conclusions drawn from considerations of geologic time. The life periods determined by chemical means may or may not be accurate, but they are as yet the only estimates of any practical value.

NOTES

1. The question of the origin of the lead in a radioactive mineral now stands somewhat as follows: In thorium minerals containing any considerable percentage of uranium the atomic weight gives hardly any evidence what part of the lead is original and what part is of radioactive origin. In uranium minerals containing no thorium, assuming that the quantities of radum B and Radium D are negligible, the atomic weight should be 206. Practically this atomic weight is never obtained and seldom approached, and the difference apparently points to the presence of original lead of doubtful origin and atomic weight, and therefore, not readily allowed for. The necessary allowance has never been attempted, at least so far as I am aware. In a considerable number of results the proportion of lead is too small to allow the atomic weight to be determined at all. There is thus here a very large error, although smaller than those previously mentioned. Also, should it be possible to be sure of the origin of the lead, which it is not, all we could be sure of is that there is a certain percentage of uranium lead. Even then there is no certainty what proportion of this lead originates from the uranium of the mineral during the period since the mineral was laid down.

2. An early and very interesting theory of variable atomic weights is that of Sir William Crookes (Address to the Chemical Society, 1888), bearing specially on the rare elements. It will be noticed that Sir William Crookes, unlike others holding similar theories, rightly and logically postulates a concomitant variation of properties. Concerning that address many, I think, are of opinion that the last word has not yet been said on the place of the rare elements in the Periodic Table.

Results of Volcano Study in Hawaii

By T. A. Jaggar

THE Hawaiian Observatory was founded in 1912 by the Massachusetts Institute of Technology, and financed in large measure by business men in Hawaii. Its publications have been systematic volcanologic and seismometric bulletins, and two larger reports, as well as numerous special articles. The scientific work has been done by Mr. T. A. Jaggar, director of the station, and Mr. H. O. Wood, associate. Preliminary announcement of results at the end of the first five years of work reveals discoveries which may be of interest to science at large, and some of these discoveries are briefly reviewed here.

NATURE OF HAWAIIAN GASES AND FLAMES

The gas collected from a blowing-cone in the lava pit of Kilauea in 1912 by Day and Shepherd contained dominantly sulphur dioxide, carbon dioxide, and nitrogen, subordinate amounts of the combustible gases, sulphur, carbon monoxide, and hydrogen, and only 4 per cent of water vapor. The 79 per cent of SO_2 CO_2, and H_2O could not, to the writer's thinking, be juvenile, but must in part result from union with atmospheric oxygen. Day had suggested that heat-producing reactions between such gases as free S, CO_2, and H, rising through the lava, would raise the surface temperatures so that the lava column might be at its hottest above instead of in the depths. Continuous recording and observation of flames, with experimental measurements of temperature and soundings of the lava for viscosity differences, show that this generalization is well founded, and, in addition, that atmospheric oxygen is brought in contact with the magmatic gas so as to produce abundant flames of different colors. Air is sucked down at the convectional whirlpools and cascades. It is carried downward in the liquid lava lakes by foundering of porous crusts which cannot melt in the superfused lava glass. Air is also carried down in broken wall rock, in avalanches, and by burial of old talus. Lastly, with 33 per cent volume shrinkage due to such gas reaction within the lava column as $2H_2+O_2=2H_2O$, even at high temperatures (1100° C. more or less), and with convectional gas pumping, a Bessemer furnace effect through the liquid lava may be created by indraught of air from the walls.

Of the three combustible gases H, CO, and S, sulphur is most in evidence as surface flames, carbon monoxide along with impurities may be represented by rare flames, while hydrogen probably flashes mostly to water vapor in depth. There are whitish flames occasionally seen, and intensely hot bluish to violet flames play at all times from the glowing grottoes and chimneys. Some work has been done in an effort to photograph the flames with color filters and panchromatic plates, and there is a promising field here for the study of flame spectra.

NATURE OF A LAVA COLUMN

While it was known many years ago that some of the Hawaiian lava pools were shallow, few observers have imagined that the liquid lava rising 600 feet during a year within a pit much deeper than that would be found by sounding at the end of the period to be only 45 feet deep, though still fully liquid at the surface. This was the case at Halemaumau, the inner lava pit of Kilauea, in January, 1917. Sounding was accomplished by plunging a steel pipe into the lava lake at several different locations, and always the pasty bottom was found at fewer than 50 feet of depth, with due allowance for the angle of immersion. This discovery, however, checked perfectly with the results of continued observation and survey which had repeatedly made record of shoals appearing in the lava, and of cascades from the liquid lake into marginal voids and over submerged ledges, after a period of subsidence. These hitherto unexplained facts at once became intelligible when it was realized that the lava column in reality is a semi-solid body filling the true crater from side to side, while the liquid lake is a gas-heated froth maintained through conduit holes 'honeycombing the upper part of the harder column. The basin of the lake is a shallow saucer, and convectional circulation keeps the liquid lava in motion. The famous islands and benches are of the bench magma, or semi-solid substance which forms the bottom of the liquid lake.

THERMAL GRADIENT OF LAVA LAKE

With batteries of seger cones encased in iron netting and strung on a wire, which in turn was placed within long steel pipes, measurement was made in 1917 of the thermal gradient of the liquid lava pool. Individual temperature measurements were also made of the fountaining grottoes at the margin of the lava and of flaming chimneys through blowing-cones above it. The highest temperatures, about 1,350° C., were found in this air zone of free oxidation of gases the fountaining lava reached a maximum of about 1,180° C., the bright lines of the lake surface were at about 1,000° C., while just below the surface the temperature was 100° lower. From here to the bottom of the lake 40 feet down there was rising temperature. A thick lower stratum of the shallow lake showed uniform temperature between 1,100° and 1,200°. This lower stratum probably represents reheating due to oxidation of gas in contact with air carried down by foundering crusts. The fall in temperature toward the lake surface from the bottom up, which in the middle region amounts to 70° C. per meter, is due to surface radiation aided by gas expansion. The localized surface heating is due to surface oxygen and completion of reactions between rising unstable gas mixtures.

DERMOLITH AND APHROLITH

The writer has proposed these terms for fluidal lava and block lava respectively, called *pahoehoe* and *aa* by the Hawaiians, because, as the result of the investigations here recorded, he believes dermolithic *versus* aphrolithic process to represent respectively the liquefaction of lake magma and the gas expansion solidification of bench magma. The dermolithic basalts of Kilauea crater, characterized by wrinkled skins, have sufficiently adjusted and diminished their gas-bubble content to solidify from without inward. The aphrolithic or *aa* lava, a "foam-stone," which is expelled in a Mauna Loa flow, cools from within outward by expanding gas suddenly released from solution, and the lava disintegrates into rough units. Lava drawn up from deep within the Kilauea lake tended, on sudden cooling, to effloresce in aphrolithic fashion. An island which rapidly rose from the lake bottom proved to be typical *aa* or aphrolithic lava. The most satisfactory feature of the discovery that the Hawaiian lava column is probably stiff within the mountain is the correlation now possible with such volcanoes as Pelee, Bogoslof, or Tarumai, which exhibited hard domes and spines instead of lava flows. The liquid or dermolithic lavas now become products of surface fusion induced by escape of gases from solution in a very stiff intratelluric magma as solvent. A volcano like Kilauea, which among volcanoes exhibits maximum temperatures, probably owes the liquidity of its surface lava to the nature of its gas reactions.

CYCLICAL AND SYMPATHETIC LAVA MOVEMENTS

A complete eruption of Mauna Loa, the summit crater of which is 22 miles from the Kilauea sink and about 10,000 feet higher, consists of a preliminary summit outburst, followed, after months or a few years, by a flank discharge with lava flow. Recently the intervals between identical phases of complete eruptions have averaged something above nine years. Kilauea has shown no hydrostatic response to Mauna Loa lava, hence it was supposed they were unconnected. It will be clear, however, that if a main lava column depends for liquefaction on surface release of gas from a stiff silicate magma solution, hydrostatics plays only a superficial *rôle* while varying viscosity, differential expansion, and tidal stress control relative heights of lava in adjacent and connected conduits of different sizes.

During the complete eruptive period of Mauna Loa, 1914-1916, five seismic spasms in that volcano, two of them accompanied by eruption, were responded to in the active lava pit of Kilauea by a series of pronounced risings of increasing duration, followed by sudden subsidences of increasing amount, as shown on the accompanying chart. The last and greatest subsidence of June 5th, 1916, happened at Kilauea just at the close of the lava flow which culminated the eruptive period of Mauna Loa, and the lava column thereafter rose steadily for seven months on the Kilauea side of the system, the Mauna Loa side being sealed. There is good reason to suppose that similar sympathetic relations have existed in previous eruptions. There were no seismometric and volcanometric data on those occasions, and quantitative records are essential to establish such correspondences.

Another line of investigation, based on analysis of lava-tide charts plotted for four and a half years, and on a study of the imperfect records from 1865 to 1911, indicates that there are larger semi-annual and smaller semi-monthly variations in the height of the lava column, after making due allowance for local interferences and longer term cycles, which vary strikingly with a time curve constructed to express the relative amounts of the forced nutational strains in the globe attributable respectively to sun and moon. Mr. H. O. Wood computed this curve, and the writer executed the lava measurements with alidade or transit for the years 1912, 1913, 1914, 1915 and 1916. It is possible that the longer term cycles vary with a strain curve of free nutation (Chandler) due to variation of latitude.

SEISMIC INDICATION OF VOLCANIC ACTIVITY

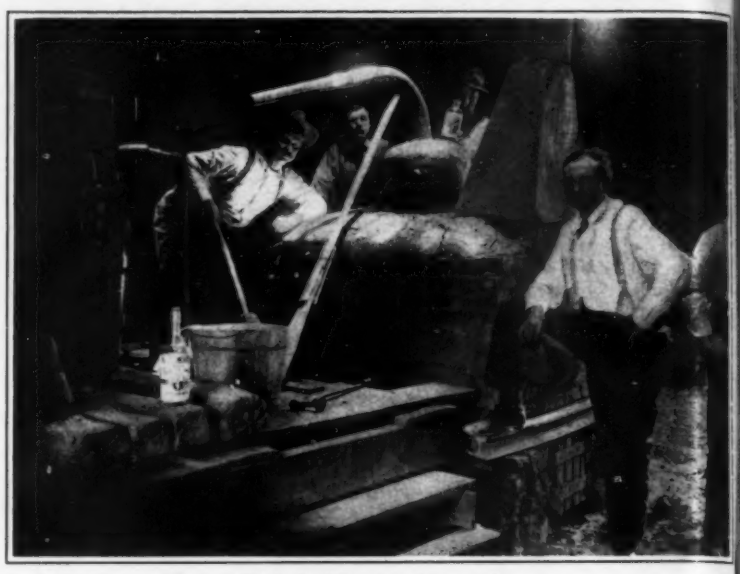
As stated above, there were earthquake swarms accompanying and preceding the outbreaks of Mauna Loa, and there have been similar groupings of local shocks accompanying the ups and downs of the Kilauea lava column. In addition, there are volcanic vibrations and extraordinary tiltings of the ground, the latter both periodic and prolonged, which promise intensely interesting data concerning the movements of the hard lava underground. Remembering the permanent surface deformation determined geodetically after the San Francisco earthquake, and after the eruptions at Usu and Sakurajima, in Japan, the writer believes, from experimental evidence, that a volcano station is most advantageously placed for critical seismometric investigation of the progress of such displacements. The coordination of deep magmatic movements with the earthquake problem is the profoundest enigma of geology.

Charms Against Earthquakes

DR. W. L. HILDBURGH describes some Japanese charms connected with earthquakes in the April issue of *Man*. When an earthquake occurs a person in danger should repeat, over and over again, as rapidly as possible, the word *Manzairaku*, signifying "Ten thousand years of happiness." The term is used between persons as a form of congratulation; either on account of its congratulatory significance, or by some play of words, its use during an earthquake is intelligible. The Japanese for some obscure reason, believe that a privy is the safest place of refuge during an earthquake. This may be due to the fact that the place is a haunt of spirits, and on leaving, one should throw there a piece of iron, like a nail, as a protection. On the whole, Dr. Hildburgh is disposed to connect the belief regarding the privy as analogous to the well-known principle of dressing children in dirty clothes as a charm against the Evil Eye or similar dangers.—*Nature*.

Photos by the Gilliams Service

A small home-made still



Illicit distilling on a large scale

Moonshine Land

Where Slackers and Deserters Are Finding Refuge

EVER since our entry into the great war Moonshine Land, that extensive and dubious district in the mountainous regions of some of our southern states, has attracted more than usual attention because it has become a hot bed of slackers and deserters.

Over and over again the government officers, while they have been trying to raid illicit distilleries, have found in the same neighborhood deserters and slackers for whom Uncle Sam has been looking, in some cases, for months.

As one illustration; down in the northwestern part of Alabama, where the government recently purchased and set aside a large tract for a game reservation moonshiners, slackers and deserters have been recently giving federal officers no end of trouble.

The men are reported to be working in perfect accord, and are said to be conspiring in many ways to elude the government representatives.

There is more than one reason why moonshiners are particularly active just now. The prohibition which Congress has placed on the manufacture of whiskey during the war has increased the price of the stock on hand and has naturally added greatly to the value of the moonshine goods.

Then again, the adoption of prohibition by so many of the southern states has naturally made the legitimate whiskey of commerce, on which the government revenue tax has been paid, difficult to obtain in prohibition neighborhoods and the moonshiners, whenever possible, have been quick to profit by this condition.

Among the southern mountains the terms moonshiner and moonshining are not used. An illicit distiller is called a blockader, and his business is known as blockading and the product of his still as blockade liquor.

Just as the smugglers of old England called themselves freetraders, thereby proclaiming that they risked and fought for a principle, so the moonshiner considers himself simply a blockade runner dealing in contraband.

There are two kinds of blockaders, big and little. The big blockader makes unlicensed whiskey on a fairly large scale. He may have several stills, operating alternately in different places, so as to avert suspicion. In any case, the still is large and the output is quite profitable.

The owner himself may not actively engage in the work but may furnish the capital and hire confederates to do the distilling for him, so that personally he shuns the appearance of evil. These big fellows are the ones who seek collusion with the small-fry of government officialdom, or failing in that, instruct their followers to kill on sight. Fortunately they are rare.

The little, and much more plentiful, moonshiner is a more interesting fellow. There is nothing between him and the whole power of the Federal Government except his own wits and a well worn Winchester, or old-fashioned muzzle loader which has been passed down in the family, an heirloom from previous generations.

Apart from his moonshining this man is a good enough citizen, and usually has some standing in his own community. Toward all the world, except the revenue officers, he is a right good fellow. Usually he is very poor, ignorant and when captured has no friends at court. His apparatus is crude in the extreme and his output is usually small.

Although in the eyes of the law he is a criminal he is soundly convinced in his own mind that the law is unjust, and that he is only exercising his natural rights in making illicit whiskey.

Every man in the big woods of the southern mountains is a jack of all trades. His skill in extemporizing utensils, and even crude machines out of the trees that grow around him, is of no mean order. If this were not so there would be no way for him to find the material out of which to build his distillery.

While it is possible for one man to run a small moonshine still it would be a hard task for him to do so; besides there are few mountaineers who could individually furnish the capital, small though it be. So usually two or three men combine in the business of "blockading." They will "chip in" five or ten dollars apiece, and purchase a second-hand still, if such is on the market, otherwise a new one, and that is all the apparatus they have to pay money for.

If they should be too poor to even go to this expense,

How the moonshiner lives

they will make a retort by inverting a half barrel or an old wooden churn over a soap-kettle, and then all they will have to buy is a piece of copper tubing for the worm. In selecting a location for their clandestine work the first essential is running water. This can be found in almost any ravine; yet out of a hundred known spring-branches, only one or two may be suitable for the business, most of them being too public. In a country where cattle and hogs run wild, and where a good part of every farmer's time is taken in keeping track of his stock, there is no place so secret but that it may be visited at any time, even though it be in the depths of the great forest, several miles from human habitation.

The moonshine-still must consequently be placed miles away from the residence of anyone who might be liable to turn informer; for while practically all the residents of Moonshine Land, even including the mountain preachers, are very indulgent in the matter of blockading, yet personal rivalries and family jealousies are rife among them, and it is not uncommon for them to inform against their enemies in the neighborhood.

In selecting a place to set up his still the moonshiner is careful to notice every track, every pass whether of beast or man and to "read the sign" with Indian like facility. Usually some little side branch is chosen that runs through a gully so choked with laurel and brier and rhododendron as to be quite impassable save by such moving and crawling as must make a great noise.

Frequently a faint cattle trail follows the backbone of the ridge above it, and this is the worker's ordinary highway in going to and fro; but the descent from ridge to gully is seldom made twice over exactly the same course, lest a trail be printed direct to the still-house.

Sometimes this place is enclosed with logs, but oftener it is no more than a shed, built low, so as to be well screened by the undergrowth. A great hemlock tree may be felled in such a position as to help the masking, so long as its top stays green, which will be about a year.

Back far enough from the still-house to remain in dark shadow when the furnace is going, there is built a sort of nest for the workers, barely high enough to sit up in, roofed with bark and thatched all over with browse. Here many a tiresome hour of night is passed when there is nothing to do but to wait the cooking.

Now and then a man crawls to the furnace and pitches in a few billets of wood, keeping low at the time, so as to offer as small a target as possible in the flare of the fire.

Such precaution is especially needed when the number of confederates is too small for efficient picketing. Around the little plot where the still-shed and lair are hidden, laurel may be cut in such a way as to make a *chaval de-frise*, sharp stubs being entangled with branches, so that a quick charge through them would be out of the question. At most two or three days' work will build a shed house and equip it ready for business without so much as a shingle being brought from the outside.

The still once established, the next thing the moonshiners do is to make arrangements with some miller who will run the risk of grinding the sprouted corn. For be it known that corn which has been forced to sprout is a prime essential in the making of moonshine whiskey, and that the unlicensed grinding of such corn is an offense against the law of the United States, no less than the distillation.

To those who are accustomed to the rural conditions as they exist in the North and much of the West, where there is only one mill to many farms, it would seem that the moonshiner would be up against it to find a miller who would do the work for him, and so he would under these conditions; but in the southern mountains about every fourth or fifth farmer has a tiny mill of his own that will grind a bushel or two of corn in a day. Thus with such mills in plenty the moonshiner usually has but little difficulty in getting his work done.

It is impossible for the moonshiner to age his whiskey. In the first place he is too poor to wait; in the second place his product is so small, and the local demand is urgent; in the third place, he has enough trouble to conceal, or run away with a mere copper still to say nothing of barrels of stored whiskey. So when the stuff is yet warm from the still it is taken by confederates and quickly disposed of. As for purity, all of the moonshine

whiskey used to be pure, and much of it still is; but every blockader knows how to adulterate, and when one of them does stoop to such tricks he will stop at no half way measures. Some add washing lye, both to increase the yield and to give the liquor an artificial bead; then they prime this abominable fluid with pepper, ginger, tobacco or anything else that will make it sting.

As a rule the mountain people have no compunction about drinking. Men, women and children drink whiskey in family concert. In spite of this, taking the residents of moonshine land as a whole they are an abstemious people. Comparatively few of them see liquor oftener than once or twice a month. The lumber jacks and townspeople get most of the output, for they can pay the price.

Moonshine whiskey until recently, when the war put up the preice tremendously, sold to the consumer at from $2.50 to $3.00 a gallon, now it is said to fetch as much as $2.00 a quart, and is greatly adulterated at that.

A Frequent Cometary Visitor

THERE is one comet that returns toward our Earth more often than any other of these numerous "spectres" of the darkened firmament. This is "Encke's Comet" which has but recently visited us again. Not that it really made a personal call on our Earth but as it drew near its "perihelion," its nearest point to the Sun (about 32,000,000 miles), we had a chance to become better acquainted with it than when it is at "aphelion" (about 380,000,000 miles). Of several so-called "short periods" comets its time of completing its orbit, from perihelion to aphelion and back again to perihelion, is the shortest, about 3⅓ years (3 years and 109 days). Indeed, its "period" is much shorter than that of the next most frequent cometary visitor, the comet named "Temple I," which has a "period" of 5⅞ years.

Although discovered at Paris in 1786 by Messier and Méchain, and afterward rediscovered respectively by Caroline Herschel and Pons, it was named after J. F. Encke (1791-1865), who computed its orbit in 1819. Not only is this comet peculiar in its period, but it exhibits other peculiarities, for example, its diameter which approximates 300,000 miles when 130,000,000 miles from the Sun, shrinks to a diameter of about 14,000 miles when it arrives at perihelion. Another of its peculiarities is that Encke's Comet is not regular in the time of each of its tours from perihelion to perihelion. During the nineteenth and twentieth centuries it has varied from 1,213 days to 1,204 days. This cometary irregularity has been attributed to a possible resistance by the ether through which it is passing. Thus, Encke's Comet presents an exception to the general rule of such bodies for since its periodicity was discovered in 1819 it has been constantly increasing its speed and decreasing its period. It is, therefore, a "comet of mystery." The recent "perihelion" of this remarkable comet occurred on March 24th, 1918.—CHARLES NEVERS HOLMES.

Some Peculiarities of Color and Their Bearing on Photographic Work*

By A. J. Bull

LIGHT falling upon an object to be photographed is reflected from the surface, but we are concerned not merely with the light which is reflected at the actual surface of the object, but also with the light which penetrates a little way into the substance and is reflected as it were from the inside of the material. The depth of penetration of the light may be very slight, but provided that the light penetrates the object at all and goes a little way through the substance, that light is subjected to the effect of selective absorption. If the whole of the visible light is reflected, then we say that the object is white, but it may very well happen that some of the constituents of the light are absorbed, and then we speak of the light which is sent back to the eye as colored, because it is white light less some of its colored constituents.

Colors produced in this way by selective absorption have varying properties, and I will show an experiment to demonstrate some of the differences that exist. We take a solution of potassium permanganate, and place it in the path of a beam of light, and in the path of another beam we place a solution of one-tenth the strength. There is a difference in brilliancy and also in color. By stopping down the lens through which the brighter beam is passing we bring our two images roughly to the same brilliancy, and then it is seen that there is a decided difference in color: one is a blue while the other is pink. Here is a case where the color of the light which has passed through an absorbing medium differs in quality with the strength of that medium.

If we now place in the path of one beam a solution of picric acid and in the path of the other beam a solution diluted ten times with water, we shall see that while the dilute solution allows more light to go through, the change in color is not so great, and if we reduce the amount of light passing through the lighter solution, we see that the difference in color produced by the effect of dilution is but slight.

If a piece of yellow film is placed half-way across the spectrum, then it will be seen that the reason why the dilution of the picric acid solution produces so little change in its color is that yellow is opaque to blue and to violet light, while it is, on the other hand, very transparent to the remaining constituents of white light. In such a case, when by selective absorption one constituent of the white light is completely removed and the remaining constituents are allowed to pass in considerable quantity, then not much change is obtained in the color, whether the solution is strong or weak.

We are aware, of course, that all ordinary colors are produced by removal of some of the constituents of white light by selective absorption; but it does not follow that this occurs very often. In a large number of cases the absorption is more or less gradual, and then the effects are more complicated.

The fact that yellow is a compound color and is produced by the abstraction of the blue-violet constituents of the white light, and that the removal of those constituents is very often complete, is a phenomenon which has many bearings on photographic work.

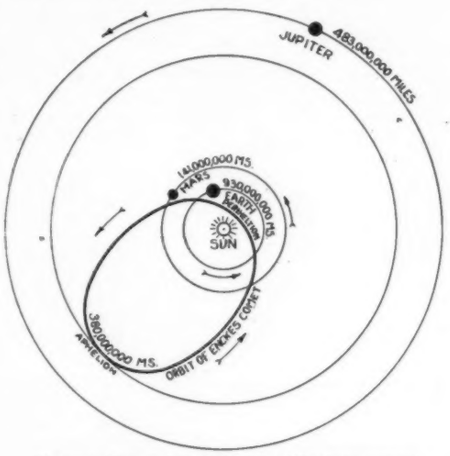


Diagram illustrating the orbit of Encke's comet

Safe-lights, which are used in the wet-collodion process, are a bright yellow. Wet plate work is conducted under modern conditions of process engraving in a room which is well lighted, and this is possible partly because of the efficiency of the safe-lights. It is easy to construct safe-lights which remove the whole of the blue-violet and ultra-violet light, and allow to pass all those brilliant red, yellow, and green rays which give to the eye the effect of yellow. The efficiency of orthochromatic filters also is affected considerably by this point. Ordinary yellow filters for orthochromatic work are very efficient. The amount of light of longer wave-length—the red and green light—which they obstruct is very slight, but by using the dye in suitable quantities one may grade down the blue light just to the extent which is required.

I show now a photograph of some buttercups on a wet collodion plate as used by the process engraver. The brilliant yellow flowers are rendered as black, the reason being that the petals of the buttercups do not return to the camera any of the light to which that plate is sensitive, and this point is, perhaps, a little remarkable because it is only occasionally that one gets the whole of the blue and violet light so completely removed that it cannot produce any effect at all upon the ordinary photographic plate.

Now we come to a group of subjects which show this effect most markedly. These are objects which are varnished with a yellow varnish. In these cases the actual varnish lying on the top of the wood or the oil-painting very often plays a most striking part in the result. The varnish is itself somewhat yellow, but the light which falls upon it is partly reflected at the topmost surface. One thus gets a considerable amount of surface reflection, much of it being regular. That light is white, it, therefore, contains the blue and violet constituents which affect the ordinary photographic plate, so that an object of this type, when photographed on an ordinary plate, is recorded chiefly by part of the surface-reflected light. If, however, we photograph by the longer wave-lengths, we get a record of what is under the yellow varnished screen.

I have one case in which the object can only be photographed by light which passes through it. It is a piece of old Kauri gum containing some insects. The rendering with the ordinary plate shows no sign whatever of the insects. It is the complete removal by selective absorption of all the light to which the ordinary photographic plate is sensitive which produces this result. When, however this same object is photographed by the longer wave-lengths of red, yellow, and green, then we are able to obtain a record of what is inside the substance, and the insects are plainly visible. There is always that difference between the two cases. When one is photographing an object by reflected light, then whatever kind of light is used for taking the photograph, one can always obtain some sort of result, but as in the photography of insects in amber, where the light has to penetrate through the material, if selective absorption by a yellow-colored material occurs, then it may mean that the light which is ordinarily used for photographic work is removed. Although some of these instances may seem commonplace, I emphasize them because the peculiarities of yellow are not those of many colors.

Let us now turn to the question of greens, which do not act in quite the same way as yellow. If we take any green dye and place it in the path of the light, we shall find that, while it produces its effect by selective absorption—that is to say, by the removal of some of the constituents of the white light—there is, nevertheless, the difference that it does not transmit the whole of the green light.

That is quite a common characteristic of green, more so than one might at first suppose. If the greens that occur in nature are examined it will be found that in practically every case they do not return anything like the whole of the green light which falls on them back into the air, and in many cases, especially if we were to try the experiment with such green leaves as are available in December, we should find that the reflection of green light only amounts to 4 or 5 per cent. In no case does the reflection of green from the green leaf amount to more than one-third. Yellow and blue make green, but the green filters for photographic work do not admit of the same efficient construction as do the yellow filters, and the reason is that we have not in the case of blues a color which acts in the same way. In the case of the blues and the greens we do not have a complete absorption of any one color and an efficient transmission of the remaining colors. We are here dealing with a class of colors which are apt to change according to their strength.

I show a photograph on the screen of a landscape in which the leaves on the trees give no evidence of detail upon the exposed under surfaces, whereas the upper surfaces are only too well exposed. Here we are dealing with the effect of surface reflection. The leaves are spread out in the way that nature has provided so as to catch as much of the sun's rays as possible. The solar light, falling on this leaf, is reflected to some extent at the surface, and that surface reflection, comprising as it does every kind of light which the sun has sent to the leaf, has among its components just those particular wave-lengths which affect the photographic plate, and so this is well exposed. But the light which penetrates the leaf has undergone selective absorption, and it so happens that almost the whole of the light which affects the photographic plate has been removed by that selective absorption. If we photograph the same object again, using light which comprises green among its components, we shall be able to photograph the detail of the undersides of leaves, and in this case we no longer have the violent contrast between the surface reflection and the dark underside of the leaf, because the green light is being photographically recorded.

There are two cases of selective absorption in marked contrast. We have, on the one hand, the case where some light absorbed is absorbed very completely, and other light is transmitted almost in its entirety. On the other hand, we have the more common case where no one constituent of the white light is completely absorbed and no one constituent completely transmitted. It is very fortunate—indeed, there is probably a great natural law behind it—that it is in some of the useful cases that there is complete absorption and high transmission. Complete absorption is only combined with high transmission in those cases in which it is the shorter wave-lengths which are absorbed and the longer wave-lengths which are transmitted. In all other cases we have nothing so definite.

*A lecture delivered before the Royal Photographic Society; reported in *Brit. Jour. of Photog.*

Wartime Rations for Animals

WAR conditions have created radical changes in feeding the animals in the New York Zoological Park. Coarse corn bread has proven a very successful product, and it is being cooked in large batches daily. This bread is made in square loaves of about two and a half feet square by four inches thick. Some of the animals prefer it to any other food—and this is particularly marked among the bears.

What the Road Really Costs*

By Geoffrey De Holden-Stone

On this question one must ask at the outset, for certain fundamental things to be admitted as such, without further question. One is that auto-transport is already by all odds the cheapest, the most time-saving, the most fluid and therefore circulatory; in a score of ways the most convenient, and from every aspect, the most fecund of prosperity; also, that it still stands—and long will stand—to be improved in every one of these inherent qualities.

The second is—and no very hard admission, since most of us are already agreed upon it—that we must reconstitute British roads for stresses far beyond those of passenger auto-traffic, if we are to obtain permanently the benefits of commercial auto-transport.

From both these postulates we arrive at the third, that money spent—almost to any amount the prospect will bear—upon highway reconstruction is really the finest of national investments.

But the fourth corner of agreement, being purely economic, is often forgotten, indeed rarely remembered; that after all, the road—no more than the railway or the ship—is not a productive affair like a factory. Its profits, vast as they may be, are not immediate at best, nor more than approximately measurable in figures; nor can they ever be more than indirect, however assuredly manifest. So that to be truly profitable, worked to the best advantage, it must be asked to show double earnings, somehow!

So it comes to this, that the still and unproductive machine-base called a road, can only earn single profits, let alone double, by increasing, from the outset or progressively, those of its machine of transport-traffic—itself no more productive, however, wondrously creative—and this *only by decreasing the working costs of that machine to the minimum.*

Thus it is evident that those working costs, after a certain early point has been reached, are really chargeable to the road.

It must therefore be this consideration, I submit, that should really govern all our deliberations as to road-construction and reconstruction. Otherwise, we shall establish an early and irremovable limitation to our actual traffic earnings; and worse still, thus narrow the margin for cheapening our transport.

For with the traction-efficiency-maximum and friction-resistance-minimum standard fixed for us on the level steel rail or plate, we have nothing to give away.

Instinctively, from the time a yoke of oxen was first swung round a hill that it could not possibly climb, we have endeavored to run our roads to within such gradients as would allow comfortable and therefore effective haulage. But seldom, if ever yet, have we analyzed the matter with the sole view of translating the just-effective into the L.S.D.-efficient. Yet this, with auto-transport in view as a commercial proposition—national at that—is what we are now forced to do.

We shall not, therefore go far wrong, I submit, if from the outset we mentally charge all working costs to the road that exceed the known test-bench consumption-costs *plus* those of overcoming the aforesaid friction-resistance-minimum. These are the inevitables. It would seem to be a counsel of perfection; extreme and at first sight unpracticable. But if we are not after perfection, why go on: what then is the use of any standard?

The point is, auto-transport is coming, or not coming, just as *we* contrive to bring it, and as we grasp it, or not. It is going to be worth while bringing, or not, according to how we make all ready for its arrival on any scale. And it is going to be the biggest part of our national transport, the greatest factor of the regeneration of our national prosperity, of our living amenities, *or not* solely according to the margin afforded by the working conditions that we provide beforehand for the reduction of its working costs.

And since all this glowing futurity, all these working conditions, lie right in the road itself to begin with, in the sort of road we make it, we cannot, I should say, regard our task too seriously for posterity's sake and our own. Else we win no war!

Let us prospect it this way. Suppose we have a section of main road, a mile in length, that carries or is supposed to carry, one thousand auto-wagons a day; the gross laden weight of which average only four tons each; and that this represents the traffic average for three hundred days in the year. There we have an aggregate of 1,200,000 tons. These figures, by the way, are not uncommon fact.

Now on the assumption of 20 per cent fuel-efficiency it follows that any extra effort will cost more petrol; and that the consumption will be less efficient by roughly 20 per cent of that extra amount, in all probability; even if, on a rising gradient, we agree—as we should not, strictly speaking—to disregard the well-known progressive-resistance effect of load-inertias, by way of addition. Let us then assume a gradient of only 1 per cent on this mile; and again—quite *as an arbitrary figure, not* a fact—that the extra petrol consumed amounts to only one-tenth of a gallon daily per wagon. The annual extra amount then comes to only the nice little total of 30,000 gallons!

Now at only 1s. a gallon we have the sum of £1,500 extra that that 1 per cent of up-grade has cost our tonnage. Reduced to farthings, we find that it has cost each ton just 1.2 farthing more. That is to say, it has cost transport more, enabled the traffic, and hence actually the road, to earn 1.2 farthing *less*.

And that sum, be it noted, is more than a quarter of the coin for which it has been calculated that a locomotive on the railway, bound by no stoppages or other restrictions, can haul a ton of goods *one hundred miles*. Assumption or not as to actual consumption, we may be quite certain that the extra amount is not only over one-hundredth of a gallon—still more the thousandth it should be to bring that 1.2 farthing to a parity with the railway—but very much in fact. So the inevitable conclusion is, again, not only that we have nothing to give away, but can only look for our saving margin to our greater door-to-door directness of auto-transport, and to other like advantages; whereby we stand to profit by comparison with the current defective conditions, the delays and cost in ton-hours, that prevail on the railway. To anything but the latter's merits.

But as these defects stand to be improved away, we see where we are; begin to appreciate with what intense care we must legislate for every aspect and item of our road-constitution, and the organization of our auto-transport, whereby working costs may be saved. For it is these savings that are the real earnings—albeit negative profits, like the little girl's pins—of the road.

Let us look at the matter from this other angle. Charging this extra cost of £1,500 to the road, we should really capitalize it at 5 per cent. That represents a sum of just £30,000.

Clearly, then, that £30,000 is the sum we ought to be justified in spending to save that 1 per cent grade and that 1.2 farthing per ton, if the traffic-prospect exists, or—as is possible—can be road-created.

Now, for all we know to the contrary—until we have examined the matter, as if we were about to invite tenders for the work—we may be able to get it done for £15,000 or less. There at once, in our cost-savings, we have the amortization of that £15,000 clearly in sight. That self-evident fact, I suggest, discloses at least one secret—perhaps the most important one—of the profits of the road, and the profitableness of auto-transport as a general proposition.

Everyone who has read Mr. C. L. Freeston's delightful books of Alpine auto-touring, and noted his telling illustrations of the grades of Alpine roads, can hardly fail to be impressed by the supreme importance that has evidently been given to the matter of gradient-reduction by Continental road engineers.

But these, from our immediate point of view, are instructive in another way. The matter demands survey, it seems to me, on the basis of crow-flight distance between point and point. We may iron out the gradient by 1 per cent, or 2, 3 or 4, from the original natural road-gradient as we find it, or project to lay it. That shows us a saving as heretofore demonstrated, truly. All very well. But the point is, are we going to lose it, or not, *by the extra mileage consumption of the extra distance involved* by all that interesting zig-zagging? For it is on the balance of difference that our actual petrol-saving—and road-profits and tonnage profits—depends. Nor must we forget to add on the *debit* side, the matter of ton-hours for extra time spent in covering the additional distance.

That balance, at any rate, will be our deciding factor. If it cancels out to no saving, or even shows a minus loss, we have then to consider whether the traffic, in expected amount—or by sheer necessity of a highway just in that locality and direction—is going to bear that permanent loss. Here it would seem that the question will become one of whether it will really pay to reconstitute that stretch of road, or lay one, at all. But against that we have the very practical argument of transport available—albeit permanently costly—or none at all. For the railway it is certain, will not be taken that way. So the final conclusion being more or less affirmatory—the prospect and conditions should be bad indeed to make it otherwise—we must in such case, make the best of it, and so engineer the road as to cut our loss as cheaply as possible. So long as we realize that the road *is* losing just there.

There is, of course, the chance that this loss may be offset by the fact that uphill going is down-dale returning, with the motor cut out, the traffic coasting and the fuel consumption thus wholly stopped; not only cancelling out that of the uphill travel, but *actually giving us our ton-mileage on that entire stretch one way for nothing.*

Now this is not only a prospect, but a probability under most average conditions. By careful organization of transport service routes—and here, if nowhere else, one can see the advisory powers of the State taking a useful hand—it can be made a certainty. Yet here again is matter for the utmost care. The tendency—as seen in the U. S. A.—is not so much for auto-transport service to run to and fro on the same road, but to circulate in a continuous run back to the starting point, race-fashion: no matter to what queer shapes the intended service may distort the actual "circuit." All the more so that the physiology of transport—like that of the body—has established the fact that circulation is ever so much more effective for internal commerce than mere arterial to-and-fro spur-lining.

With these considerations in mind, it can be seen, I suggest, that a circulatory transport service might be so organized as to run in that direction that mostly gives the grade downhill. Theoretically, even if the circuit lay almost entirely on a plateau—like that of Boulogne for instance—the idea sounds ridiculous. In practice nothing is more constant that the difference found in the amount of petrol consumption according to the direction taken. So before any road is laid—let alone reconstituted—the route can be roughly prospected by some sort of auto-vehicle—best of all, one of the commercial type intended for the service—as to this very point. For if the general lie of the circuit shows—or can be directed to show—merely longer down-grades than up-grades, we score. For with gravity working our mileage *gratis* in the former case, length tells for ton-miles in fuel values, more than adverse grade percentages tell against them in the latter case.

The next fundamental enquiry is, what loss the road stands to make—or what not to lose—on the score of road-surface resistance, which as we have agreed, would be directly chargeable to the tonnage traffic. I suggest, therefore, that we can ascertain this on a comparison with the grade cost in fuel consumption already noted. For admitting the 1.2 farthing per ton to be quite arbitrary, even so it is no less good as a basis-constant. It is true, too, that the findings as to tractive effort required for various surfaces per ton-load, seem to vary among experts in this country. But even so, the steel surface represents the accepted minimum in any case; so as our object is to find eventually what one pound of tractive force is worth in petrol, on the grade or on the level, I also suggest that these aggregate differences hardly matter; and that we can also take *arbitrary* figures for the minimi, so long as, on the one hand, we have our level-steel-rail-or-plate standard, or its grade equivalent, and on the other, can get the pound-unit in either case, into a common value-denomination with the grade unit, in terms of fuel consumption per ton carried.

The connecting link is the assumed fact that the tractive force required to move a 1-ton load on steel up a 1 per cent grade, is 20 pounds: this effort being clearly needed in addition to whatever further force may be required to overcome such resistance as is due to the nature of the road surface. There, then, we clearly have our 1.2 farthings per ton as the fuel value of 20 pounds, and the consequent pound-unit as with exactly .06 farthing in fuel-consumption on that 1 per cent grade.

Taking next—merely as a basis of argument—the level steel-rail-or-plate minimum-surface friction standard to be as much as 8 pounds we need not go cube-rooting to find its value. Simple proportion shows that this pound-unit value is as 8 to 20 of the other one, that is to say, .024 farthing. That is the irreducible cost in fuel per pound of tractive effort: so for our ton moved we get .024×8 as agreed—or .192 farthing as the irreducible minimum cost. To which, of course, every grade unit percentage adds its quota of .06 farthing per pound unit of required force, or 1.2 farthing per ton moved.

The point, however, is road surface friction cost. Recently, one department of the University of California estimated that the force required on a plain level concrete surface was 27.6 pounds as against 64.3 pounds for average good macadam. Personally, I think this latter figure is higher than the usually-accepted British estimate. But as has been said, with the unit-values clearly established as they have been relative aggregates and their differences do not matter, as they only serve as illustrations of principle. Here then, at any rate we get additions respectively of 19.6 and 56.3 pounds. Or, multiplied by our unit value .024, .47 and 1.35 farthing respectively, on the level; and, 1.176 farthing and 3.378 farthing for a grade of only 1 per cent.

Actually, from nearly half a farthing, to more than three of the most important coins in the world to oil-

*From the *Auto-Motor Journal.*

brokers, women, and Welsh drapers, *because of the surface differences of a perfectly good road, for every ton that travels it for ever.*

Let us see what it amounts to, merely on the lower scale of dead level—how much of it can we ever count upon in this changeable delight which is England?—for those thousand four-ton autos for only one year. I make it exactly £587 10*s.* Or more painfully, £4,222 10*s.* along a stretch hardly to be told from an east-country level. It seems to be worth saving for the cheapening of our auto-transport commerce, as a paring of hard-set service. For capitalized at 5 per cent with amortizations of loans to be thought of, it means anything between £11,750 and £84,450. Surely, if as has been seen, we should be justified in spending so much capital, how much better to save it; since it is clearly there to be saved by taking thought beforehand?

And the £587 10*s.*—£4,222 10*s.* petrol value and the £11,750—£84,450 capital outlay are both for *one mile only.*

Now our enthusiastic American trunk auto-road builders and highwaymen are all agog with the wonders of concrete. One reads of little else in their movement. But nowhere, that they are reinforcing it by so much as a length of steel wire netting. One does not remember, either, that their auto-race-tracks were ever made of anything but lumber or plain dirt. Now we at least have had our concrete at Brooklands—albeit unreinforced—not worse engineered, I imagine, than the best of their concrete roads. And we have seen—some did see—what the intermittent traffic of racing cars, averaging 1 ton each, perhaps, did to that concrete. So it really seems, in the light of that experience, that what the perpetual traffic of a thousand 4-tonners would do to any hundred miles of concrete trunk road would be a shame.

But something of the kind, super-reconstitutive, we have got to have. Both the £587 10*s.*—£4,222 10*s.* perpetuality and the £11,750—£84,450 permanency, are really chargeable to the tonnage-traffic. So that the road has to earn by not spending, gain by not losing.

Let us hark back to our steel bases. Our less than-a-fifth of a farthing to a farthing-and-a-fifth works out at £240 and £1,500 exactly. Or capitalized, at £4,800 and £30,000.

We have just seen that the lowest—and practically impossible—dead-level capital figure, the minimum that could not be saved further in petrol, running upon concrete, was £11,750.

And I remember that the most extreme estimate of the most expert critic of the Plateway was *under* £10,000 *a mile for its construction.*

Internal Ballistics

In a lecture on "Internal Ballistics" at the Royal Institution, Lieut.-Col. A. G. Hadcock said it has long been recognized that a gun is a single stroke engine, and a very inefficient one at that. Although it is impossible to obtain by direct means an indicator diagram connecting the pressure in the gun with the corresponding position of the shot, it is possible to draw such a diagram if certain facts are known. A gun is, however, essentially an internal combustion engine, and many of the facts connected with it correspond much more nearly to a gas, oil or petrol engine than to a steam engine. Indeed, there are many analogies between a gun and an ordinary internal combustion engine. The processes which take place in a gas or oil engine are comparatively simple, the gas or fuel is introduced with the proper amount of air into the cylinder, compressed, and then exploded or burnt there at more or less constant pressure, the hot gases expand and the piston is forced along the cylinder.

The propellant, which forms the charge for a gun, always contains in itself the necessary ingredients to ensure complete combustion. The weight of explosive necessary to produce any given amount of energy is therefore considerably greater, since it also contains the oxygen necessary for combustion, than the weight of gas or oil which would produce an exactly similar amount of energy when combined with the proper proportion of air. It is this fact which renders a gun an inefficient machine when it is compared with other internal combustion engines. The addition of air or oxygen is unnecessary, but since gunpowder, cordite and the kindred propellants will not begin to burn in a vacuum it is essential that atmospheric pressure should exist in the gun chamber. When the charge is ignited in a breech-loading gun a somewhat complicated series of operations occur which may be thus enumerated—the gun being supposed loaded:

(1) The charge begins to burn at atmospheric pressure.

(2) The pressure rises and forces the copper driving band which surrounds the projectile to give rotation into the rifling grooves.

(3) The charge still burns and the pressure forces the shot along the gun.

(4) The charge is completely burnt.

(5) By the expansion of the hot powder gases the shell is forced further along the bore and out of the gun.

One of the most important of these processes is the forcing of the driving band into the rifling, and the subsequent behavior of the charge depends greatly on the initial resistance of the driving band to being forced into the grooves. The greater this resistance the larger will be the proportion of the charge burnt before the band is fully engraved, and the higher will be the pressure when the band is fully engraved.

Most artillerists have, so far, treated the problem of internal ballistics either (*a*) as if the gun were a closed vessel for each position of the projectile, as in Centerval's and Mansel's methods, or (*b*) that the pressure for each position of the projectile bears some definite relationship to the pressure which would have been developed had the charge been burnt in a closed vessel of capacity equal to that of the chamber of the gun. This method of treatment has been adopted by Gossot, Heffernan, Charbonnier, and others.

Neither of these assumptions is correct, because in a closed vessel no linear motion is given to the charge, while in a gun a considerable amount of energy is absorbed by the charge having to be put into action. Thus we find that the pressure on the breech of the gun is higher than that on the projectile. The pressure on the base of the shot is that due to putting the shot alone into motion, while the higher pressure on the breech is due to the additional work required to be done in putting also into motion the weight of the gas or charge. Direct experiment proves this to be the case, and usually the difference in pressure on the breech and on the base of the shot amounts to between one and two tons per square inch.

Another way of looking at this fact is to remember that when any fluid under pressure is put into motion a certain amount of the pressure is lost. The amount so lost depends on the velocity. In our case we have the gas under high pressure; part of it is in contact with the shot and moving at the same velocity as the shot; the remainder has a gradually less linear motion until at the breech end of the gun there is no relative motion. It follows then that the forward part of the gas which has a considerable velocity will have less pressure than that part in contact with the breech end of the gun.

In a gun the space behind the projectile continually increases as the projectile travels along the bore; thus each portion of the gas as generated may, for our purpose, be considered to expand and do work on the shot. In doing so it would lose heat, but, at the same time, the charge continues to burn and additional heat is given to the already generated gases, thus our hypothetical expansion is neither adiabatic nor is it isothermal. For simplicity we suppose that no heat is imparted to the wall of the bore; but any loss from this cause is accounted for in the factor of effect.

The investigation is hedged round with difficulties, and the assumptions made by the various artillerists, who have given so much thought to the subject still left something unaccounted for. A key to the more correct solution seemed to me to be by Clerk Maxwell's diagram of isothermal lines. In Maxwell's figure each isothermal line corresponds to the expansion of gas at some definite constant temperature. These curves give therefore the relationship between the pressure, volume and temperature, and knowing any two of these the third can be immediately determined.

If now, instead of temperature, we consider these curves to relate to some definite fraction of the charge which has been converted into gas, the gas expanding according to a definite law, as though no more of the charge would be burnt, but that the proper amount of additional heat from the unburnt part of the charge was imparted to the expanding gases, then a set of lines differing slightly in character from those in Maxwell's figure are developed. They are, in fact, lines of equal combustion, but for shortness, and in order to retain the Maxwell simile, I have called them isopuric lines. The trace of each isopyric line shows the expansion curve of the gas then generated, on the supposition that the remainder of the charge remained unburnt.

For each isopyric line, if the position of the shot in the gun is known—that is to say, if the volume is known—then the pressure can be at once determined, or if the pressure is known the position of the shot can be found.

Many people think that if the resistance between the shell and gun could be prevented a large amount of additional energy could be given to the projectile in the shape of extra velocity. But whilst frictional resistance certainly consumes a proportion of the energy given off by the powder it serves a most useful purpose, and enables the powder to burn and develop its power. Were it not for this resistance only small-sized powder could be used, and it would have to burn very much in the same way as that assumed by Robins, *i. e.*, that all the charge is burnt, and a high pressure obtained before the shot moves. The final muzzle velocity would consequently be much restricted.

Although the resistance along the bore is important, it is not so important as the initial resistance of the driving band to being forced by the gas pressure into the rifling grooves and thereby being engraved. The band resistance varies inversely as the caliber of the gun, *i. e.*, the pressure per square inch on the base of the shot would be much higher in a 3-in. gun than in a 15-in. It depends also on the width of the band and its maximum diameter. As the form of these driving bands has been fixed more or less arbitrarily, and in many cases to meet individual ideas, they differ very considerably both as regards general form and their proportions, so that it is difficult to obtain a working formula to fit every case. It is therefore necessary to ascertain by statical means the resistance of each type of service band. This is done by making a short cylinder rifled in the bore at one end and slightly enlarged in the bore at the other. A plug representing the shot and fitted with a driving band of the exact shape is introduced. The cylinder is fitted with a steel screwed plug, and the base of the shot with a cup leather. Hydraulic pressure is then applied through a pipe screwed into the plug; and the diagram showing the pressure relative to the motion of the shot is engraved on a brass plate by means of a pressure indicator placed on top of the cylinder. The brass plate is moved by the shot. The actual static pressure required to force the band into the rifling begins at a very low pressure, and gradually increases until it reaches a maximum when about $\frac{2}{3}$ of the length of the band has been forced into the grooves, and then diminishes until the pressure shows a nearly uniform reduction towards the muzzle. During the time the driving band is being engraved the charge is rapidly burning, and the gas pressure rises from atmospheric pressure to four, five, or six tons per square inch, and the velocity of the shot at the end of this short period may be considerable. The gas pressure is always much higher that the static hydraulic pressure necessary to engrave the band, and it follows that a considerable fraction of the charge is consumed while the shot is being put into motion and the band fully engraved.

After the whole of the charge has been burnt, ordinary adiabatic expansion takes place, so that, at the muzzle of the gun, the mean pressure is reduced to about 4.5 tons per square inch, and the shot leaves the gun with a velocity of about 2,910 ft. per second

Cross-Hairs for Optical Instruments

A correspondent of *The English Mechanic* proposes a material for use for cross-hairs in optical instruments that is easy to procure and handle, and which should be very convenient for amateur instrument makers and experimenters. He says:

"May I make a practical suggestion to people interested in telescope cross-hairs? It is stated on page 169 that platinum wire may be had of the fineness of .000975 inch in diameter; I do not doubt its strength and costliness, but in my 'doings' I have come across a fairly strong fiber which is from .000027 to .000041 inch in diameter—perhaps even less than that. This is the individual fiber of serpentine asbestos, which under the microscope is incomparably finer than the spider threads of the microscope ocular, much smoother and straighter, and it would appear to me, as far as I can judge the matter, that a small block of natural asbestos mineral of this kind would provide an optician with a practically inexhaustible store of ready fine cross-hairs for eyepieces. Be it noted, also, that some of these fibers exist in great length, but fortunately such is not required as a rule for the purpose under discussion. I have subjected these fibers to very rough treatment between two glasses for placing them on the microscope stage, but, except for accidents, most of the components of the bunches have kept a quite startling straightness, thus indicating that they are not so 'touchy' as spider threads."

"Cellulon," A New Wood Pulp Fiber

There has, says the *Board of Trade Journal*, been much discussion in the German Press during recent weeks concerning a wood pulp fiber named "Cellulon," for which large claims are made as an efficient substitute for jute, cotton and other fibers. It appears to be certain that the fiber is not made by spinning long strips of paper run off reels through water in the manner which German paper textile substitutes have made familiar. The accounts agree in describing the process, or processes, as a direct manufacture from wood pulp. H. M. Consul-General at Zurich, who has presented a report to the Foreign office, states that the method employed operates on the same general lines as that used for artificial silk manufacture—that is, by squeezing pulp under high pressure through small holes in plates.—*The Engineer.*

Variation of the common sunflower *H. annuus*

First figure: A form selected from the group with light-tipped rays, obtained in 1916. The dark color is chestnut red. Mrs. McClintock of the University of Chicago, named it "Halo." Second figure: *Helianthus annuus*, collarette variety, parallel to the well known collarette dahlia. The first collarettes were obtained in 1915, and are figured in *Gardener's Chronicle* (London), Nov. 6, 1915, and the *Journal of Heredity*, Sept., 1916. The present figure shows an improved form, obtained by crossing the original collarette on a chestnut-red form with light bases of rays. Third figure: Plant of 1916 crop, extracted from red doubles, but showing no trace of the "double" feature. The numerous long flat rays are in striking contrast with the sunflower in center of next page. Sunflowers vary not only in the shape of the rays but also in their position. They may be level with the disk, or directed more or less forward, or else backward as if drooping. Fourth figure: A variety marked nearly as in the first figure, but the rays much more obtuse. There is also a light ring around the disk. This was chestnut red, very bright and distinct; grown in 1916 from seed received from Messrs. Sutton and Sons, of Reading, England. The form is derived from our original cultures, but has been bred and selected by Sutton.

The Red Sunflower

The Story of a New and Highly Decorative Blossom

By T. D. A. Cockerell, Professor of Zoölogy, University of Colorado

THE original red sunflower was found by Mrs. Cockerell close to our house in Boulder, Colo. Among many wild sunflowers growing by the roadside was a single individual in which the rays were suffused with chestnut-red. It was at once recognized that here was an opportunity for the production of a new horticultural type, as well as for scientific investigation. The plant was dug up and removed to the garden, where it continued to bloom until the end of the season. The sunflower being an annual, and at the same time infertile with its own pollen, the only possible way to preserve the new variety was by crossing with the more ordinary sorts. This was done, and in the following year many plants with reddened rays were produced. These could now be crossed on one another, and a permanently red strain obtained. Today the red sunflower may be found in gardens in Europe and America, and even in Australia, New Zealand, and South Africa, all the plants being descendants of the single wild sport which appeared at Boulder in 1910.

Sunflower garden at Boulder, where the red sunflowers were developed. Mrs. Cockerell is half hidden among her plants.

The current manuals of botany, at least in America, pay very little attention to variation. In the new edition of Dr. Britton's *Illustrated Flora*, descriptions of varieties have been omitted altogether. This may be necessary in a work of such scope, where only the briefest outline of the flora is possible. Anything like a full treatment of varieties would double the size of the volumes, with necessarily a corresponding increase in cost. Nevertheless, we must some day have a work on American botany treating the subject with that fullness of detail which is not uncommon in modern European floras. Only by close attention to the phenomena of variation can we expect to understand the evolution of plant species, and at the same time to develop those new horticultural forms which may add so much to the wealth and beauty of farms and gardens.

We sometimes hear of the "creation" of new kinds of plants by the breeder. The plant breeder can utilize only what nature provides. In the first instance he must search for materials to serve his purposes—for plants possessing the characters he desires to impart to his new horticultural forms. In the case of the red sunflower, the variation was so conspicuous and remarkable as to arrest the attention at once; yet even so, the chances of the plant being saved were rather slight. It is perhaps, no exaggeration to say that a thousand red sunflowers might bloom in as many American towns, without anyone thinking it worth while to save the seed and carry on breeding experiments. When we come to valuable new varieties of less conspicuous plants, such as wheat or oats, the present chances for isolation and preservation are infinitesimal. Not even the professional botanist is, as a rule, trained to observe minute differences; he often actually prides himself on ignoring everything below the grade of a species. Yet these small differences may carry the potentiality of an increase in yield sufficient to feed thousands of people, or of some new form of beauty to gladden the eyes of garden lovers in two continents. Thus it would doubtless be a paying proposition for this country to employ a number of skilled botanists to make a minute study of the variations of plants, and set their observations forth in a series of cheap volumes which would stimulate a far wider and more intelligent interest in the subject.

Species-hybrid *Helianthus annuus* (red) x *petiolaris*, the latter being the seed parent. Grown in Boulder in 1917. The *H. petiolaris* parent was raised from seed collected in Oklahoma.

Although in one sense the breeder creates nothing, yet he does virtually create in the sense that the new combinations he produces are in effect and to all practical purposes actual novelties. This may be illustrated by the history of the wine-red sunflower. If we take rays of the chestnut-red variety and soak them in acidulated alcohol, we get a solution which is not chestnut-red but wine-red. The red color is in fact due to a carmine pigment dissolved in the sap; it appears chestnut because on an orange background. Knowing this, the problem was to get it on to another background, in order to bring out the wine-red color. We should have preferred white, but no white-rayed sunflowers are known. The nearest approximation is a very pale yellow, which we have found as a wild sport, but which has also been long in cultivation. In England it is called the primrose sunflower, because the color is like that of an English primrose. Crossing the full red sunflower with the primrose variety, we got, as expected, offspring like the red parent. These were raised in the greenhouse during the winter, to save a year, and were crossed with one another. Their offspring included chestnut-red, plain orange, primrose, and the expected wine-red, which is simply red on a primrose background. All this appeared, not only as expected, but nearly in the proportions which were deduced on theoretical grounds. Thus we had the following:

	Obtained	Expected according to theory
Chestnut-red	71	69
Wine-red	25	23
Orange	19	23
Primrose	8	8

Sunflower aristocrats

Wine-red sunflowers; they were in a dull blue vase, the combination of colors being especially lovely. A spray of *Gypsophila* (baby's breath) has been added.

Two Texas species—hybrids

Helianthus annuus x *argophyllus* shows the silvery *argophyllus* foliage and bud. This particular specimen has vinous colors on a buffy ground, and is a second generation cross. *Helianthus* (*argophyllus* x *annuus vinosus*) x *annuus vinosus*.

Helianthus annuus x *cucumerifolius* displays a characteristic color pattern similar to that of the hybrid with *petiolaris*.

Sunflower aristocrats

A semidouble with long curled rays, obtained in 1915. The so-called double sunflowers are those in which the disk-florets have taken on ligulate features, paralleling the condition normal in the dandelion.

The wine-red, thus obtained according to definite rules of breeding, was an entirely new color variety, yet its qualities existed separately in its grandparents. Just how it all works out, we will not attempt to explain in this article, but it is in accordance with the laws discovered by Gregor Mendel many years ago, and fully set forth in many recent works as "Mendelism."

It is often supposed that when a breeder desires to produce new plants, he has only to cross different species possessing desirable characters. The chances of success in doing this vary very much according to the kinds of plants used. Among the hothouse orchids, hybrids between different genera are common, and as they can be propagated by division, they are capable of being increased without losing their characters. Annual sunflowers, which are propagated by seed, offer much greater difficulties. The annual species so far experimented with readily cross, and produce fertile seed. This seed gives rise to hybrid plants, which are themselves so nearly sterile that they cannot be offered horticulturally. On the other hand, all the varieties of the common sunflower, *Helianthus annuus*, seem to cross without any impairment of fertility. Even the great so-called Russian sunflower produces quite fertile hybrids with the prairie sunflower, which some botanists have regarded as a distinct species. Nevertheless, even where there is abundant or sufficient fertility, complex crosses break up into a number of different types when propagated by seed, and are capable of being extracted pure only after labor extending over several years. This is why apples cannot be propagated successfully by seed, although the varieties are readily preserved and spread through grafting. Dahlias, which can be propagated vegetatively by means of tubers, possess many constant varieties, although these same varieties would rarely come true from seed. Could we increase the annual sunflower in the same manner as the dahlia, we should now have about fifty distinct recognizable horticultural forms, any or all of which could be placed on the market. Three species-hybrids are illustrated, the *annuus* parent being in each case a red variety. One of the accompanying illustrations shows an *annuus* x *argophyllus* cross, *H. argophyllus* being a robust plant from Texas, remarkable for its white silky pubescence. The hybrid shows the silky character distinctly, and is a very pretty thing. The *H. annuus* x *argophyllus* hybrid was independently produced by Sazyperow in Russia, in this case with the object of obtaining a plant resistant to rust and other diseases. Another illustration represents the cross *H. annuus* x *cucumerifolius*, the latter being also a species from Texas, but small and branched. The *H. cucumerifolius* used were cultivated forms. A similar cross, except that the *annuus* used was not red, had been made previously in Europe. *H. annuus* x *petiolaris*, new in 1917, is also shown. It is a beautiful plant, much branched and with numerous heads. It remains in full flower later than the common sunflower.

The remaining figures show different variations of the *H. annuus* type, some peculiar for the form of the rays, others for the color and markings. Marking factors which control the distribution of the colors, are independent of the color factors, and may be inherited through plants which have no red in the rays at all. Thus, although *H. petiolaris* is without red, it has a very distinct pattern or marking factor, which appears in the hybrids with the red *annuus* as a red ring at the bases of the rays.

Sunflowers are cultivated very easily, and anyone may experiment with them. When carrying on breeding experiments, it is necessary to put bags on the flower heads before the flowers open, and carry the pollen to the flowers by hand, repeating the operation several times on each head. If this is not done, bees bring pollen to the flowers and it is impossible to tell which varieties the pollen parents belong to. In Colorado, at least, we find it necessary to bag the heads at the seeding stage also, to protect them from the birds.

All the work in breeding sunflowers has been done by my wife, who has devoted her summers to it for a number of years past.

Red sunflowers of the 1915 crop, showing the form with twisted and curled rays; a very attractive type. Such forms can be obtained both in the chestnut-red and the vinous colors.

"Little Wonder"—A Red Sunflower. A pretty vinous form obtained in 1917; named by Mrs. Cockerell. The heads are rather small and the bases of the rays are a lively lemon-yellow.

Helianthus annuus variety *primulinus*, the pale yellow type used in the first cross to obtain the wine-red (variety *vinosus*) form. The primrose variety was introduced to horticulture by Messrs. Sutton and Sons, of England, in 1889, it having occurred as a sport among the ordinary orange plants a few seasons previously. The plant here figured is an improved form, with two rows of rays, recalling the star dahlias.

Alcohol from Horsechestnuts

FERMENTATION experiments indicated that 100 kilos. of dry horse-chestnuts will yield 27-28 liters of alcohol, which, though lower than the yield obtainable from maize would warrant their industrial use under present conditions. They contain about 0.5 per cent of potassium and 0.15 per cent of phosphoric acid, both of which are indispensable yeast foods.—*Feuille d'Information*.

Radiation and the Electron—II*

New Facts Require a Revision of Classical Theories

By R. A. Millikan, Ph. D., Professor of Physics, University of Chicago

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It is possible, however, to go a certain distance toward a solution and to indicate some conditions which must be satisfied by the solution when it is found. For the energy $h\nu$ with which the electron is found by experiment to escape from the atom must have come either from the energy stored up inside of the atom or else from the light. There is no third possibility. Now the fact that the energy of emission is the same, whether the body from which it is emitted is held within an inch of the source, where the light is very intense, or a mile away, where it is very weak, would seem to indicate that the light simply pulls a trigger in the atom which itself furnishes all the energy with which the electron escapes, as was originally suggested by Lenard in 1902,[9] or else, if the light furnishes the energy, that light itself must consist of bundles of energy which keep together as they travel through space, as suggested in the Thomson-Einstein theory.

Yet the fact that the energy of emission is directly proportional to the frequency ν of the incident light spoils Lenard's form of trigger theory, since, if the atom furnishes the energy, it ought to make no difference what kind of a wave-length pulls the trigger, while it ought to make a difference what kind of a gun, that is, what kind of an atom, is shot off. But both of these expectations are the exact opposite of the observed facts. *The energy of the escaping corpuscle must come then, in some way or other, from the incident light.*

When, however, we attempt to compute on the basis of a spreading-wave theory how much energy a corpuscle can receive from a given source of light, we find it difficult to find anything more than a very minute fraction of the amount which the corpuscle actually acquires.

Thus, the total luminous energy falling per second from a standard candle on a square centimeter at a distance of 3 m. is 1 erg.[10] Hence the amount falling per second on a body of the size of an atom, *i. e.*, of cross-section 10^{-15} cm., is 10^{-15} ergs, but the energy $h\nu$ with which a corpuscle is ejected by light of wave-length 500 $\mu\mu$ (millionths millimeter) is 4×10^{-12} ergs, or 4,000 times as much. Since not a third of the incident energy is in wave lengths shorter than 500 $\mu\mu$, a surface of sodium or lithium which is sensitive up to 500 $\mu\mu$ should require, even if all this energy were in one wave-length, which it is not, at least 12,000 seconds or 4 hours of illumination by a candle 3 m. away before any of its atoms could have received, all told, enough energy to discharge a corpuscle. Yet the corpuscle is observed to shoot out the instant the light is turned on. It is true that Lord Rayleigh has recently shown[11] that an atom may conceivably absorb wave-energy from a region of the order of magnitude of the square of a wave-length of the incident light rather than of the order of its own cross-section. This in no way weakens, however, the cogency of the type of argument just presented, for it is only necessary to apply the same sort of analysis to the case of γ-rays, the wave-length of which is of the order of magnitude of an atomic diameter (10^{-8}cm.) and the difficulty is found still more pronounced. Thus Rutherford[12] estimates that the total γ-ray energy radiated per second by one gramme of radium cannot possibly be more than 4.7×10^4 ergs. Hence at a distance of 100 meters, where the γ-rays from a gramme of radium would be easily detectable, the total γ-ray energy falling per second on a square millimeter of surface, the area of which is ten thousand billion times greater than that either of an atom or of a disk whose radius is a wave-length, would be $4.7 \times 10^4 \div 4\pi \times 10^{16} = 4 \times 10^{-7}$ ergs. This is very close to the energy with which β-rays are actually observed to be ejected by these γ-rays, the velocity of ejection being about nine-tenths that of light. Although, then, it should take ten thousand billion seconds for the atom to gather in this much energy from the γ-rays, on the basis of classical theory, the β-ray is observed to be ejected with this energy as soon as the radium is put in place. This shows that if we are going to abandon the Thomson-Einstein hypothesis of localized energy, which is, of course, competent to satisfy these energy relations, there is no alternative but to assume that at some previous time the corpuscle had absorbed and stored up from light of this or other wave-length enough energy so that it needed but a minute addition at the time of the experiment to be able to be ejected from the atom with the energy $h\nu$.

Now the corpuscle which is thus ejected by the light cannot possibly be one of the free corpuscles of the metal, for such a corpuscle, when set in motion within a metal, constitutes an electric current, and we know that such a current at once dissipates its energy into heat. In other words, a *free* corpuscle can have no mechanism for storing up energy and then *jerking* itself up "by its boot straps" until it has the huge speed of emission observed.

The ejected corpuscle must then have come *from the inside of the atom*, in which case it is necessary to assume, if the Thomson-Einstein theory is rejected, that within the atom there exists some mechanism which will permit a corpuscle continually to absorb and load itself up with energy of a given frequency until a value at least as large as $h\nu$ is reached. What sort of a mechanism this is we have at present no idea. Further, if the absorption is due to resonance—and we have as yet no other way in which to conceive it—it is difficult to see how there can be, in the atoms of a solid body, corpuscles having all kinds of natural frequencies so that some are always found to absorb and ultimately be rejected by impressed light of any particular frequency. But apart from these difficulties, the thing itself is impossible if these absorbing corpuscles, when not exposed to radiation, are emitting any energy at all; for if they did so, they would in time lose all their store and we should be able, by keeping bodies in the dark, to put them into a condition in which they should show no emission of corpuscles whatever until after hours or years of illumination with a given wave-length. Since this is contrary to experiment, we are forced, even when we discard the Thomson-Einstein theory of localized energy, to postulate electronic absorbers which, during the process of absorbing, do not radiate at all until the absorbed energy has reached a certain critical value when explosive emission occurs.

However, then, we may interpret the phenomenon of the emission of corpuscles under the influence of ether waves, whether upon the basis of the Thomson-Einstein assumption of bundles of localized energy traveling through the ether, or upon the basis of a peculiar property of the inside of an atom which enables it to absorb continuously incident energy and emit only explosively, *the observed characteristics of the effect seem to furnish proof that the emission of energy by an atom is a discontinuous or explosive process.* This was the fundamental assumption of Planck's so-called quantum theory of radiation. The Thomson-Einstein theory makes both the absorption and the emission sudden or explosive, while the loading theory, first suggested by Planck, though from another viewpoint, makes the absorption continuous and only the emission explosive.

The h determined above with not more than one-half of 1 per cent of uncertainty is the explosive constant, *i. e.*, it is the unchanging ratio between the energy of emission and the frequency of the incident light. It is a constant the existence of which was first discovered by Planck by an analysis of the facts of black-body radiation, though the physical assumptions underlying Planck's analysis do not seem to be longer tenable. For the American physicists Duane and Hunt[13] and Hull[14] have recently shown that the same quantity h appears in connection with the impact of corpuscles against any kind of a target, the observation here being that the highest frequency in the general or white-light X-radiation emitted when corpuscles impinge upon a target is found by dividing the kinetic energy of the impinging corpuscle by h. Since black-body radiation is presumably due to the impact of the *free* corpuscles within a metal upon the atoms, it is probable that the appearance of h in black-body radiation and in general X-radiation is due to the same cause, so that, contrary to Planck's assumption, there need not be, in either of these cases, any coincidence between natural and impressed periods at all. The $h\nu$ which here appears is not a characteristic of the atom, but merely a property of the ether pulse which is generated by the stopping of a moving electron. Why this ether pulse should be resolvable into a continuous, or white-light spectrum which, however, has the peculiar property of being chopped off sharply at a particular limiting frequency given by $h\nu = PD \times e$ is thus far a complete mystery. All that we can say is that experiment seems to demand a sufficient modification of the ether-pulse theory of white-light and of general X-radiation to take this experimental fact into account.

On the other hand, the appearance of h in connection with the absorption and emission of *monochromatic* light (photo-electric effect and Bohr atom) seem to demand some hitherto unknown type of absorbing and emitting mechanism within the atom. This demand is strikingly emphasized by the remarkable absorbing property of matter for X-rays, discovered by Barla[15] and beautifully exhibited in De Broglie's photographs[16]. It will be seen from these photographs that *the atoms of each particular substance transmit the general X-radiation up to a certain critical frequency and then absorb all radiations of higher frequency than this critical value.* The extraordinary significance of this discovery lies in the fact that it indicates that there is a type of absorption which is not due either to resonance or to free electrons. But these are the only types of absorption which are recognized in the structure of modern optics. We have as yet no way of conceiving of this new type of absorption in terms of a mechanical model.

There is one result, however, which seems to be definitely established by all of this experimental work. Whether the radiation is produced by the stopping of a free electron as in Duane and Hunt's experiment, and presumably also in black-body experiments, or by the absorption and re-emission of energy by bound electrons, as in photo-electric and spectroscopic work, Planck's h seems to be always tied up in some way with the emission and absorption of energy by the electron. *h may, therefore, be considered as one of the properties of the electron.*

The new facts in the field of radiation which have been discovered through the study of the properties of the electron seem, then, to require in any case a very fundamental revision or extension of classical theories of absorption and emission of radiant energy. The Thomson-Einstein theory throws the whole burden of accounting for the new facts upon the unknown nature of the ether and makes radical assumptions about its structure. The loading theory leaves the ether as it was and puts the burden of an explanation upon the unknown conditions and laws which exist inside the atom. I have already given reasons for discrediting the first type of theory. The second type, though as yet very incomplete, seems to me to be the only possible one, and it has already met with some notable successes, as in the case of the Bohr atom. Yet the theory is at present woefully incomplete and hazy. About all that we can say now is that we seem to be driven by newly discovered relations in the field of radiation either to the Thomson-Einstein semi-corpuscular theory, or else to a theory which is equally subversive of the established order of things in physics. For either one of these alternatives brings us to a very revolutionary quantum theory of radiation, that is, a theory which calls for an explosive emission of energy in units and which, therefore, has something akin to atomism about it. To be living in a period which faces such a complete reconstruction of our notions as to the way in which ether waves are absorbed and emitted by matter is an inspiring prospect. The atomic and electronic worlds have revealed themselves with beautiful definiteness and wonderful consistency to the eye of the modern physicist, but their relation to the world of ether waves is still to him a profound mystery for which the coming generation has the incomparable opportunity of finding a solution.

Waste in the Hiring of Men

According to figures collected by John Lind, assistant secretary of the National Lumber Manufacturers' Association, one large automobile concern, in 1913, lost $2,500,000 in hiring, temporarily training, and then losing or dismissing 52,445 men in order to keep a constant working force of 13,000 men. This may be an extreme condition, but it indicates the cost of changing men and shows that it pays to keep a steady permanent body of labor. This large labor turnover has now been greatly reduced by this firm. A Pittsburgh corporation with a permanent force of 16,000 men had a labor turnover, in 1916, of 187 per cent. Experts estimate that it costs from $40 to $150 to hire and train a new workman; hence this amount is lost if he is dismissed or becomes dissatisfied and leaves of his own accord.—*Machinery.*

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*Presented at the meeting of the Section of Physics and Chemistry of the Franklin Institute, and republished from the Journal of the Institute by permission.

[9] *Ann. d. Phys.* (4), VIII (1902), 149.

[10] Drude, "Lehrbuch der Optik," 1906, p. 472.

[11] *Phil. Mag.*, XXXII (1916), 188.

[12] "Radioactive Substances and their Radiations," p. 288.

[13] *Phys. Rev.*, VI (1915), 166.

[14] *Ibid.*, VII (1916), 157.

[15] *Phil. Mag.*, XVII (1909), 749.

[16] These photographs will be found also in the August number of the *Physical Review* (see Presidential address of the President of the Physical Society).

The Modern Languages Report

THE Modern Languages Report recently issued covers a very extensive field. Among its recommendations is one to the effect that the systematic and scientific study of foreign countries be encouraged by the Foreign Office. A higher language qualification, the report states, should be required in the general Consular Service and for student interpreters for the Far East. Before going out, student interpreters should receive an intensive training at home in the language and phonetics of the country to which they are allotted. The report further asks that more opportunity be given to the officers of the Army and Navy to acquire knowledge of modern foreign languages. It is asked that phonetics form part of the training of all entrants to the public service whose duty will lie in foreign countries.

The business community in every considerable center of foreign trade are recommended to combine with the education authorities to form institutes of languages; business men should encourage the study of foreign languages by apt members of their staff. Neither Latin nor Greek should be made compulsory for an arts degree in any of our universities. The new professorships and lectureships should be, some of language and philology, some of literature, some of the history of institutions, some of the economics of the five principal European countries. The number suggested is 55 professorships and 110 lectureships; the allowance for French would be half as much again as for each of the other four languages.

The report proceeds to deal with the study of foreign languages in secondary schools, and recommends that an institution be established in London, similar to the School of Oriental Studies, for the intensive study of the greatest possible number of European tongues, with the geography, resources, industries, and all valuable information concerning the minor countries. Emphasis is laid upon phonetic instruction, and upon the requirement of the aid of foreign assistants. Another recommendation deals with the study of the products, industry, trade, economic conditions of the chief European countries, and also of North and South America, Asia and Africa.

As will be seen from the above very brief abstract the report covers, as we stated, a very wide field, a far greater one than would be expected from its mere title. The language question is, perhaps, rather out of the scope of the subjects we usually deal with, but we view the recommendation with respect to our consular service with very great pleasure, in that it shows a tendency on the part of the Government offices to appoint for British consular duties British citizens only; hitherto the number of non-British subjects, and subjects who have proved to be enemy subjects, who were officially appointed to take charge of British interests abroad, was nothing less than calamitous.

Stress is laid in the recommendations upon phonetics and upon phonetics acquired in this country. A number of professors and lecturers are recommended for appointment in each center in which instruction in foreign languages is to be given. If we take the French language, for example, we suppose that care will be taken to appoint in the same centers professors and lecturers taken from different parts of France, with a view thoroughly to acquaint British students not only with perfect French, but with the accent habitual to the North of France, to Paris, to the Center and to the South of France. We may give the following illustrations to make our point clear. A British student who has not had the advantage of listening in this country to a lecturer from the South of France will not have had the opportunity to acquaint himself with the accent current in that part of the country, nor with the extraordinary use made there of the past-definite. He will have learned *nous somme allés; nous avons été*, and will be utterly lost when he hears, instead, *nous allâmes; nous fûmes*. The use of the past-definite tense in French, one of the peculiarities of the South of France method of speech, is correct; it is even painfully correct. It is a critical tense to use in writing; it acts like a maelstrom, for when once a piece of writing is started in that tense there is no leaving it, and the past-definite ending of most French verbs sounds ridiculous to other than Southern French ears. We may add the following as other, but *vice versa*, examples of the effect of accent or phonetics. A French citizen who has learned English in France from a Scotch, or from a Welsh, teacher exclusively, has no difficulty in differentiating from each other the two words *work* and *walk* among very many other instances of words having a very similar pronunciation. If, however, he come to London, he is entirely lost, as far as these two words are concerned, for the simple reason that the Scotch accentuate each letter. Not that the Scotch pronunciation has not its pitfalls for a foreigner; we defy any foreigner, even Scotch taught, to find out the orthography of the proper noun *Searl* in the Scotch pronunciation *Sarrol*. Therefore, the recommendation "that before going out student-interpreters receive an intensive training at home in the language and phonetics of the country" is, to our mind, a difficult proposition. It is not insurmountable, as we could instance, but it requires a series of good teachers. The study of languages demands, above all, on the part of the student an inclination, a bent for the particular language or languages. It has always been, and will always be, useless to attempt to teach languages indiscriminately; a selection will have to be made among the students following the whole curriculum of a class, a selection which in the first place must start from the consciousness of the student himself, although much depends in the study of languages, as in all other studies, upon the qualifications of the teacher and the amount of interest with which he can surround his work.

It is quite true that one knows a language only when one has acquired the grammar of that language, but it is quite wrong, as many authorities have stated over and over again, to commence teaching a language by loading the brain of the student with numerous rules and innumerable exceptions. Words should be taught first, then the use of these words, and we have found no better method of learning a language among the many methods in existence than the one frequently ridiculed as containing only impossible phrases. These phrases remain imprinted on the memory and have their full effect.

The report also recommends that a committee be appointed to inquire into the potentialities of artificial languages and the desirability of encouraging the development and use of one. We need not enlarge upon this. Before a universal language can be established much unification work must be done, such as the establishing of one single temperature scale, one single series of weights and measures, and so forth. French engineers having dealings with Great Britain, and knowing enough of the English language for ordinary purposes, are quite at a loss when they are given temperatures in Fahrenheit degrees, and proportions of furnace charges in hundredweights. If we could talk to our foreign friends in their own language and freely use their own measurements, business facilities would be greatly increased. This is the object of the vast program of the "Modern Languages Report," and we wish the promoters every success in their endeavors.—*Engineering.*

Correspondence

[The editors are not responsible for statements made in the correspondence column. Anonymous communications cannot be considered, but the names of correspondents will be withheld when so desired.]

"Relations of Matter and Ether"

To the Editor of THE SCIENTIFIC AMERICAN SUPPLEMENT:

In THE SCIENTIFIC AMERICAN SUPPLEMENT, June 29th, 1918, there appeared, under the above title, a speculation by C. Morris, a person whose position is not stated, but who evidently is not sufficiently well acquainted with his subject to avoid making some glaring errors.

In the first place, in his third paragraph, he asserts that "the smallest of atoms, that of hydrogen, is estimated to contain 1,800 of these [electrons], and on this basis the largest atoms, such as that of radium, must contain well nigh a half a million." If Morris will carefully peruse some of the recent books and pamphlets on the subject of atomic structure, he will learn that the hydrogen atom is supposed to consist of a central core of positive electricity—probably the unit or electron of positive electricity—around which one negative electron revolves. The atoms of other elements consist of a certain number of hydrogen nuclei united with a less number of nuclear electrons, the whole being surrounded by a sufficient number of negative electrons to balance the positive charge on the nucleus. The largest atom, that of uranium, has 92 of these outside electrons. *The negative electron has a mass equal to 1-1845 that of the hydrogen atom, and most of the mass of any atom is contained in the positive nucleus.* This all-important positive nucleus is treated last of all by Morris, who makes no provision in his hypothesis for the origin and nature of the positive electricity. The alpha particles given off by radium are not helium, as Morris states, but the nuclei of helium atoms—four positive nuclei united with two nuclear electrons; this grouping seems to be a stable configuration, and probably is a secondary unit in the building up of more complex atoms.

In his fourth paragraph he states "the electron seems to be at once a unit of matter and a unit of one of the chief energies of matter, it thus occupying a prominent double position in nature's organization"; now while we have no *proof*, we have the *strongest evidence* that the mass of the electron is all of purely electromagnetic origin, no "material" support being present, and hence the above-quoted statement is not true.

The writer's theory requires the ether to be granular; this view enjoys little or no favor today; moreover, he takes the old mechanical view of the ether and its properties; the very reason for postulating the ether was that the sum total of the properties which it must have could not be possessed by ordinary matter; hence it has been looked upon as something entirely different and distinct from matter, and in recent years the mechanical theory, "which never did have high standing with thinking men" has come to be abandoned; the tendency now is to explain all natural phenomena in electromagnetic terms, rather than in mechanical terms. For the properties of the ether under this conception, see P. G. Nutting, SCIENTIFIC AMERICAN SUPPLEMENT, May 11th, 1912. Granting that matter is a modification of the ether, it is not desirable to set up a mechanical hypothesis based on *reasoning from analogy, when no analogy exists* between matter and the ether.

The assumptions made in paragraphs six and seven of this paper have no foundation, probability, or necessity, and the writer's hypothesis is not one of "high significance" until he brings forward a great deal more in support of it. The argument given at the first of the sixth paragraph is very poor. A speculation which attempts to explain the significant facts stated in his eleventh paragraph must, under modern theories, be along entirely different lines. In the present state of knowledge, it seems to the present writer that the tentative assumption that the electron is some sort of modification of the ether is all that is necessary. We can at most only shift the question to the origin and nature of the ether; the mind of man will never be satisfied. Even those who regard the ether as indispensable admit that it is not the only possibility, and it may be that the ether does not exist; great difficulties accompany each view.

Under the modern theories of matter and of the production of light and energy by it, and of the role played in this by the ether, etc., it is very difficult to conceive how "temperature," as we understand it, could produce the results as to the change of state of the ether, as pictured by Morris; to define a new kind of "temperature" which would produce these results, would be introducing unnecessary complications. Moreover, the ether of space seems to be absolutely unaffected in this way by the radiations of the heavenly bodies; speculations along this line are rendered difficult by the unsatisfactory state of our knowledge regarding the structure of the ether and that of light, and of the exact mechanism of propagation; but it is not at all evident how Morris' hypothesis can be tenable.

EDGAR WOOLARD.

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Oxygen Tendering of Laundry Goods

THE use of perborate soaps and similar peroxide bleaching agents in laundries has been proved to produce, after several treatments, a very serious tendering which is more pronounced in linen fabrics than in cotton. On the other hand laundry treatments with caustic soda, soap and sodium silicate, or soap with scrubbing are comparatively mild in their effects. The destructive action peroxide agents is greatly stimulated by the catalytic influence of certain metallic salts. Copper is extremely active and iron in a less degree, and cloth spotted with solutions of salts of these metals is quickly tendered by perborate solutions; hypochlorite solutions act in a similar manner. The tendering of laundry goods may be attributed to the catalytic action of traces of metallic compounds in the "dirt" with which they are contaminated, and the use of all kinds of oxygenated washing agents is strongly to be condemned.—Note from *Jour. Soc. Chem. Ind.* on a paper by P. Heerman in *Chem. Zeit.*

Water Hyacinth as a Fertilizer

THE whole fresh green plant of the water hyacinth (Eichornia crassipes) contains on the average: moisture 95.5 per cent, organic matter 3.5 per cent (containing N 0.04 per cent), ash 1 per cent. The average composition of the ash is K_2O 28.7 per cent, Na_2O 1.8 per cent. CaO 12.8 per cent, Cl 21.0 per cent, P_2O 7 per cent. The material if well-rotted has about the same composition as farmyard manure except that it is much richer in potash. If the fresh green plant is stacked immediately for rotting, a very serious loss of fertilizing ingredients may take place in the liquid exuding from the heap. It is preferable, therefore, to dry the plant for several days before stacking it. Field tests have shown that water hyacinth either in the rotted state or as ash is a valuable manure on laterite soils of the old alluvium in Bengal.—Note in *Jour. Soc. Chem. Ind.* on a paper by R. S. FINLOW and K. C. MCLEAN in *Agric. Res. Inst. Pusa. Bull.*

Banish the Barberry

Common Barberry a Menace in Spring-Wheat States—Spreads Rust to Wheat and Other Grains

THE common barberry is a direct spreader of black stem rust, a terrible scourge of wheat, oats, barley, and rye in the spring-wheat districts. Common barberry on lawns and in hedges in cities and suburbs, as well as on farms, throughout these districts is a fearful thorn in the side of the wheat raiser. The presence of this

Fig. 1.—Common barberry, showing cluster cups on the leaves. Note the three-forked spines and the irregular edges of the leaves.

shrub in your yard may mean that your county or district produces less bread to feed the Nation and the Allies. While the farmers are being urged to take every cultural and protective measure to prevent rusting of their fields, owners of city yards and estates are called upon to help by digging out and destroying this spreader of stem rust.

How serious the black rust is to the bread supply of the nation may be judged from the fact that this disease was the principal factor in reducing the yield of wheat alone in North Dakota, South Dakota, Montana, and Minnesota by nearly 200,000,000 bushels in 1916. Wherever grain is raised the presence of the common barberry bush may mean less bread. Get rid of the barberry bush, and the farmers will be encouraged to practice early seeding, use of proper varieties of grain, effective soil management, and clean culture, which help keep this plight from the fields.

The common barberry bush (*Berberis vulgaris*), including the purple-leaved variety, nurses the black stem rust through one of its stages, helps it develop, and enables it to spread to the grains in the spring and early summer. (See Figs. 1 and 2.) The most direct method of attacking this rust is to keep common barberry bushes out of wheat-growing regions. No more common barberry bushes should be planted; those now growing should be destroyed by May 1st in order to protect this year's crop. Less barberry means more bread.

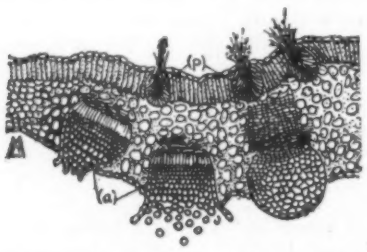


Fig. 2.—Cross section through barberry leaf, showing cluster cups containing a large amount of spores ("rust seed") at (a) and other rust structures at (p).

The effect of a single barberry bush may extend for miles. They should be dug and destroyed throughout the upper Mississippi Valley, especially in the following States: Montana, Wyoming, Nebraska, South Dakota, North Dakota, Minnesota, Iowa, Wisconsin, Illinois, Indiana, Michigan, and Ohio. Less is known about the importance of barberry elsewhere. The question will be investigated in all grain-growing regions this season.

The Japanese barberry (*Berberis thunbergii*) does not rust; it is harmless and need not be destroyed. It is more beautiful, both in summer and winter, than the common barberry and can be distinguished from it quite easily. The edges of the leaves of the common barberry are toothed, while those of the Japanese barberry are not; the spines of the common variety are usually in groups of three while those of the Japanese are usually single. (See Fig. 3.) Both have red berries, but those of the common form are borne in racemes like currants, while those of the Japanese form are borne singly like gooseberries.

Black stem-rust has not been serious in Denmark since barberry bushes were eradicated. A law providing for compulsory destruction of barberry bushes by owners was passed in Denmark in 1903. The rust had been serious periodically up to that time, but it has ceased to be destructive since the barberry bush was banned by law.

Destruction of common barberry bushes is required by law in North Dakota and Manitoba. These laws were passed in 1917. It is too early to know their effect on the rust, but it is safe to say that if neighboring States follow the example we shall have more wheat for the Allies and ourselves.

Most good farmers know that proper cultural practices will reduce the severity of rust attacks. Conditions should be made as favorable as possible for the grain, and as unfavorable as possible for the rust. This can be done in various ways.

Select Good Land.—Sow grains on high, well-drained land. Avoid pockets; rust is likely to be more destructive on low, poorly drained soils. The moisture does not cause rust, but enables it to spread rapidly.

Seed Early in a Good Seed Bed.—Grain which ripens early often escapes rust. Seed early on thoroughly prepared land, thus giving the grain a chance to ripen before the heavy attacks of rust occur. A week's difference in ripening often means the difference between good crop and a poor one.

Fig. 3.—Japanese barberry. Note the simple spines and the entire edge of the leaves. This barberry does not rust.

Avoid Excessive Applications of Manure.—Overfertilization with nitrogen is likely to cause heavy, weak straw development and delayed ripening. When such plants rust they usually crinkle badly and yield poorly.

Destroy Wild Grasses.—Many wild grasses are pernicious breeders and spreaders of rust and should not be allowed to grow. Squirrel-tail grass (wild barley), quack grass, slender wheat grass, western wheat grass, and the wild rye grasses often rust badly, even in years when grain crops do not rust severely. This enables the rust to persist more easily from one season to another. It also enables the rust to spread more widely in any given season. Clean cultivation is one method of fighting rust.

Use Early Maturing or Resistant Varieties.—Some varieties mature early and escape rust. For this reason Marquis wheat may ripen before rust becomes severe in the spring-wheat area. Wherever early maturing varieties otherwise desirable are available, they should be used.

Resistant varieties are rare. Many durum wheats do not rust severely, and where these have been tried successfully they should be used. Kanred, a hard winter wheat of the Crimean group, is grown in Kansas and does not rust as heavily as common varieties.

Breeding and selection work to secure rust-resistant varieties is being done by the Department of Agriculture in cooperation with several States. Although no seed is yet ready for distribution, progress is being made.

Details have been omitted in this brief discussion. Further information, however, can be obtained by writing to the Department of Agriculture, Washington, D. C., or to any State experiment station.

Zirconia and Zirconia Apparatus

ZIRCONIA is not only highly refractory, but it also resists the action of acids and alkalis, is a bad conductor of heat, and having, like quartz, a very low expansion coefficient, is able to bear abrupt temperature changes. Apparatus made of zirconia incline to cracking, unfortunately. To a certain extent this tendency can be met by suitable additions to the zirconia; but most mixtures are more fusible than zirconia. According to E. Podszus, the zirconia should first be melted and ground to an exceedingly fine powder with the aid of the mills to which we drew attention in a paragraph on "Keramic Objects" (*Engineering*, September 7th, page 249). A paste is then made of the zirconia with water and some organic binding agent; when compression can be applied, no binding agent is needed. The moulded articles are burnt at 2,300 degrees C. or higher temperature; if the temperature cannot well be raised above 2,100 degrees C., some phosphoric acid or boric acid may be added to the paste; these additions would be expelled again at 2,100 degrees C. The great difficulty is to melt the zirconia. For this purpose Podszus (*Zeitschrift für Angewandte Chemie* 1917, pages 17 to 19), embeds the carbons of a vertical arc in the zirconia. A little zirconia turns into carbide which covers the lower electrode; this carbide conducts the current, and a small cavity is formed round the electrodes. The carbons may then be pulled further apart; the arc burns quietly, and the zirconia melts on the walls of the cavity. With an arc of 50 amperes to 100 amperes at 220 volts, finally 30 cm. in length, Podzsus claims to fuse several kilograms of zirconia in half an hour; as he refers to French patents, however, the information is perhaps not so complete as it might be. The arc really burns first between the upper carbon and the liquid zirconia, although the latter is practically an insulator. Thoria is melted in a similar way. With the aid of a Lummer-Kurlbaum pyrometer the melting-point of zirconia was, in three determinations, found to be 2,950 degrees, 2,950 degrees and 3,000 degrees C. The oxide was not reduced by hydrogen at 2,200 degrees C., and rods of zirconia wound with tungsten wire and heated to the highest temperature in a high vacuum did not show any loss by volatilization. The pure zirconia is almost white, though generally yellowish with traces of iron, and also discolored by long heating; it is almost as hard as corundum, and difficult to disintegrate; when hard steel balls are used in mills, some iron is taken up, which can be extracted again by hydrochloric acid, however. Moulded zirconia articles should be burnt at 2,300 degrees or 2,400 degrees C., as mentioned. The burning is effected in a furnace provided with zirconia walls and a rotating flame fed with a blast of air or oxygen. Coal gas and oxygen easily give 2,400 degrees and 2,500 degrees C. in this furnace; with the aid of petroleum and oxygen temperatures of 3,000 degrees C. might be attained. Podszus has used such a furnace for 200 hours without essential repairs. Crucibles of pure zirconia will resist even fusing caustic alkalis for some time; the shrinkage of the burnt articles may amount to 20 per cent, however. Whether or not prolonged heating leads or a partial reduction of the zirconia oxide to a black suboxide, as Ruff suggested, remains uncertain. We might mention another paper by Podszus on "Reaction Velocity and Grain Size" (*Zeitschrift für Physikalische Chemie*, 92, pages 227 to 237, 1917) on this occasion. He finds that burnt zirconia, thoria and alumina, which are considered insoluble in hydrochloric acid, become distinctly soluble when they are most finely disintegrated and heated to about 60 degrees C. with efficient stirring; after 40 hours 1.7 per cent of zirconia, 1.5 per cent of thoria and 5 per cent of alumina were dissolved.—*Engineering.*

Temperature Sensitive Paints

TEMPERATURE sensitive paints is the term applied to chemical compounds that are subject to color changes at a comparatively small rise in temperature. These paints are occasionally used for indicating a dangerous rise in the temperature of machine bearings, electric generators and other apparatus where excessive heating has to be avoided. Double iodide of mercury and copper is normally red, but turns black at about 87 degrees Cent., becoming red again when the temperature falls. Double iodide of mercury and silver is normally of a light primrose yellow, but turns to a dark orange or brick red at about 45 degrees Cent. It becomes yellow again on cooling—and it may be heated and cooled an unlimited number of times without losing its curious property, providing it is not overheated.—*The Engineer.*

A New Form of Mounting for Large Reflectors

By Russell W. Porter

With the increase in size of telescopes difficulties of flexure arise that become very bothersome. In the fork form, even with the heaviest of castings, the fork will bend in all positions of the instrument. With the declination axis horizontal the bending produces its maximum effect (Fig. *I*) and stars will shift on the plate the entire amount in declination, of the angle *a*. When the declination axis is in the meridian (Fig. *II*), the flexure is there just the same, but so applied as not to shift the field of view.

Therefore the yoke is resorted to. The telescope tube is hung between the two bearings instead of outside of them as in Figure *III*, but it will be seen that in order to gain this rigidity a part of the northern heavens has had to be sacrificed. The writer submits a mounting wherein extreme rigidity is found and the entire heavens retained.

The axle of the polar axis disappears. Rather, the upper bearing is expanded to the size of the equatorial ring, see Figure *IV*, which is a perspective drawing showing the position of the instrument when directed at the zenith. This equatorial ring *A* rests on rolls or ball bearings, *b*, *b*, distributed around its rim. To the equatorial ring is bolted the ring *B* which in this case is an hour circle with its thrust bearing in the polar axis at *a*. Inside the equatorial ring is swung the declination ring *C*, carrying the tube and optical train. One quarter of ring *A* is cut away to allow the tube to swing clear from horizon to horizon. The only counterpoising is to restore the symmetry about the polar axis, disturbed by the removal of this part of ring *A*.

The following advantages for this mounting may be noted. Extreme compactness of form, the center of weight being low and within the supports, Figure *V;* the powerful leverage obtained for applying the clock drive at the rim of ring *A;* a minimum of counterpoising; and all parts of the sky accessible.

Such a mounting, if desired, can be adapted to a closed observing room, Figure *VI*, by converting it to the Cassegrainian form, adding an extra reflection as in the Common mounting at Harvard.

The diameter ratio of equatorial to declination rings is 3/2, so that for a four-foot speculum ring *A* would be six feet in diameter. It might also be noted that the instrument becomes most stable—that is, its center of weight falls midway between its three point support—in our middle latitudes. It would be impracticable at the poles or equator.—*Popular Astronomy.*

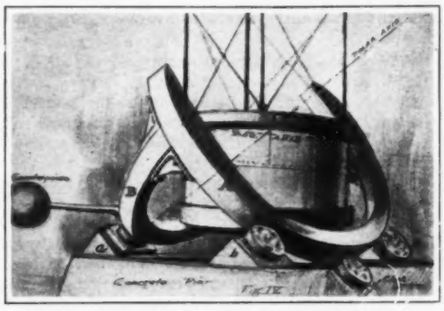
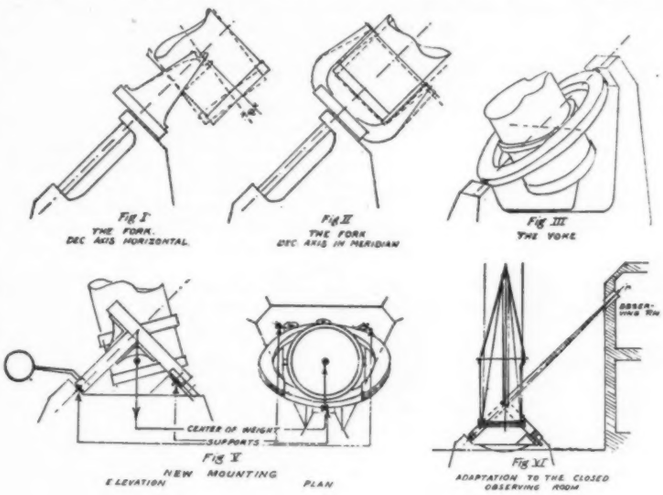


Fig. IV. Perspective drawing of proposed mounting



Geological Structure in Relation to Magnetic Disturbance

A lecture on the relationship between geological structure and magnetic disturbance, with especial reference to Leicestershire and the concealed coalfield of Nottinghamshire, was delivered before the Geological Society by Dr. A. Hubert Cox.

Before the lecture, at the request of the president, Dr. A. Strahan, director of the Geological Survey, briefly outlined the circumstances that had led to an investigation into a possible connection between geological structure and magnetic disturbances. The magnetic surveys conducted by Rücker and Thorpe in 1886 and 1891 had proved the existence of certain lines and tenters of disturbance, but those authors observed that "The magnetic indications appear to be quite independent of the disposition of the newer strata," and Dr. Strahan had not been able to detect any obvious connection with the form and structure of the Palæozoic rocks below. In 1914-15 a new magnetic survey was made by Mr. G. W. Walker, who confirmed the existence of certain areas of disturbance. It was suggested that the effects might be due to concealed masses of iron ore, and the matter was referred to the Conjoint Board of Scientific Societies, which appointed an Iron Ores Committee to consider what further steps should be taken. The committee recommended that attention should be concentrated on certain areas of marked magnetic disturbance, and that a more detailed magnetic survey of these areas, accompanied by a petrological survey and an examination of the magnetic properties of the rocks of the neighborhood, should be made. Dr. Strahan had been approached with a view to the petrological work being undertaken by the Geological Survey, and it has been arranged by the Board of Education, with the consent of H. M. Treasury, that a geologist should be temporarily appointed as a member of the staff for the purposes of the investigation. Dr. Cox had received the appointment, and his lecture would show that results of great significance had been obtained by him. The new magnetic observations had been made by Mr. Walker, and the examination of the specimens collected, in regard to their magnetic susceptibility, had been conducted by Prof. Ernest Wilson.

Dr. Cox then described the selected areas, which lay on Lias and Keuper Marl between Melton Mowbray and Nottingham, and in the neighborhood of Irthlingborough, where the Northampton Sands are being worked as iron ores. The Middle Lias iron ores, consisting essentially of limonite, which crop out near Melton Mowbray, have been proved incapable, by reason of their low magnetic susceptibility, of causing disturbances of the magnitudes observed, while the distribution of the disturbances showed no correspondence with the outcrop of the iron ores. Nor was any other formation among the Secondary rocks found capable of exerting any appreciable influence. It appeared, therefore, that the origin of the magnetic disturbances must be deep-seated.

Investigation showed that the disturbances were arranged along the lines of a system of faults ranging in direction from northwest to nearly west. The faults near Melton Mowbray have not been proved in the Palæozoic rocks, and, so far as their effects on the Secondary rocks are concerned, they would appear to be only minor dislocations. But farther north, near Nottingham, faults which take a parallel course, and probably belong to the same system of faulting as those near Melton Mowbray, are known from evidence obtained in undergorund workings to have a much greater throw in the Coal Measures than in the Permian and Triassic rocks at the surface. It appears, therefore, that movement took place along the same lines at more than one period, the earlier and more powerful movement being of post-Carboniferous but pre-Permian age, the later movement being post-Triassic. Accordingly, it is probable that the small dislocations in the Mesozoic rocks indicate the presence of important faults in the underlying Palæozoic.

The faults can give rise to magnetic disturbances only if they are associated with rocks of high magnetic susceptibility. It is known from deep borings that the concealed coal field of Nottinghamshire extends into Leicestershire, but how far is not known. Deep borings have proved that intrusions of dolerite occur in the Coal Measures at several localities in the southeastern portion of the concealed coal field, and always, so far as observed, in the immediate vicinity of faults. It has been established that dolerites may exert a considerable magnetic effect; and the susceptibility of those that occur in the Coal Measures is above the general average. Further, no other rocks that are known to occur, or are likely to occur under the area, have susceptibilities so high as the dolerites found in the Coal Measures. These facts suggest the possibility of the occurrence of dolerites intrusive into Coal Measures beneath the Mesozoic rocks of the Melton Mowbray district.

The distribution of the dolerites actually proved and of those the presence of which is suspected by reason of the magnetic disturbances, appears to be controlled by the faulting. Moreover, whereas the character of the magnetic disturbances is such that it would not be explained by a sill or laccolite faulted down to the north, in the manner demanded by the observed throw of the principal fault, it would be explained by an intrusion that had arisen along the fault-plane. The faulting itself is connected with a change of strike in the concealed Coal Measures, and the incoming of doleritic intrusions in the concealed coal field, in contrast with their absence from the exposed coal field, appears to depend upon the changed tectonic features. The change of strike is apparent, but to a less degree, in the Mesozoic rocks, which, in the neighborhood of Melton Mowbray, have suffered a local twist due to the development of an east-and-west anticlinical structure.

In view of the evidence that later movements have in this district, followed the lines of earlier and more powerful movements, it appears possible, and even probable, that this post-Jurassic (probably post-Cretaceous) anticline is situated along the line of a more pronounced post-Carboniferous but pre-Permian anticline. In this connection the isolated position of Charnwood Forest has a considerable significance. The forest is situated on the prolongation of the east-and-west line of uplift, and just at the point where this uplift crosses the line of the more powerful northwesterly and southeasterly (Charnian) uplift. Where the two lines of uplift cross, the elevation attains its maximum, and the oldest rocks appear.

The main line of faulting and of magnetic disturbance is parallel with and on the northern side of the east-and-west anticline, and the faulting is of such a nature that it serves to relieve the folding while accentuating the anticlinal structure. It is possible that this belt of magnetic and geological disturbance marks the southern limit of the concealed coal field. The results obtained by joint magnetic and geological work have thus served to emphasize the real importance of a structure which, when judged merely from its effects on the surface-rocks, appears to be of only minor importance.

A further series of observations was carried out on the Jurassic iron ores of the Irthlingborough district of Northamptonshire. The ores occur in the form of a nearly horizontal sheet of weakly susceptible ferrous carbonate partly oxidized to hydrated oxides. They give rise to small magnetic disturbances which are quite capable of detection, and these may be of use in determining the boundaries of the sheets in areas not affected by larger disturbances of deep-seated origin.

The results obtained by the joint magnetic and geological work in the two areas show that this method of investigation may be used to extend our knowledge of the underground structure. It appears also that an extension of the method to other parts of the country would yield information of considerable scientific and economic importance.—*Nature.*

Paraguayan Handmade Lace

Among the cottage industries of Paraguay is the manufacture of a lace called "nanduti" (from the Guaranian word meaning web). The articles produced, which are all made by hand, include parasol covers, handkerchiefs, doilies, boleros, mantillas, table mats, shawls, edging and insertion. The articles most popular with the foreigners in Paraguay are parasol covers, mantillas, doilies, handkerchiefs and mats, these being made of silk or very fine cotton thread. These laces, reports the United States Consul at Asuncion, appear to be superior in quality and design to similar laces made in the Canary Islands and Mexico. A handkerchief that requires from six to eight weeks to make sells in Asuncion for 100 to 150 Paraguayan pesos, and a parasol cover that takes a year to make sells for 600 to 1,200 pesos, the Paraguayan peso at the date of the report being worth about 3 cts. The lace is usually sold by peddling it from house to house, and, so far as is known, it has never been exported to any important degree. However, the leading export houses in Asuncion might be in a position to handle the trade should a foreign demand for the lace develop.

The Heavy Oil Engine—II*

A Review of the Present Construction and Future Development

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Now, when the compression starts the pressure follows the general law given at the bottom of the table, and from that certain variations follow; but we are not quite sure as to what value the exponent *S* has, although we do know it cannot be larger than 1.4. We know by experience it is not less than 1.33. So, for each one of these values I have calculated two figures—the compression pressure in pounds per square inch above the atmosphere—and you see here that to reach ignition temperature with 200° initial temperature would require 190 pounds compression minimum to 293 pounds maximum—actually somewhere between; it is difficult to fix it any closer. Whereas, if the initial temperature was 600°, then the ignition temperature would be reached in that same cylinder with a compression of only 23 pounds to 31 pounds. That brings out in the most striking fashion this fact: That if the fuel is in contact with the air in the cylinder during compression, then the amount of compression cannot be very large in any case without producing an uncontrolled pre-ignition. And, more important still, if any part of the oil and air is very warm before compression starts, then practically no compression can be carried at all. And, if you cannot carry much compression, then you have a very high fuel consumption and a very low efficiency. An engine carrying such low compressions as are here required would burn nearly two pounds of oil per hour per horsepower, whereas the best engines are today running on about four-tenths of a pound—a ratio of five to one.

Now, suppose an igniter that was itself under control, and fuel in contact with the air in the cylinder. How much can we compress it without any danger of preigniting, so that when we want to fire it can be fired with the igniter that is under control? I suggest that 200° margin is about as good as can be estimated. Let us limit the final compression temperature to something like 200° under the ignition temperature, in which case these are the allowable compressions: With 600° initial temperature, the final compression pressure is eight to ten pounds; with 200° initial temperature, 104 to 152 pounds (see table).

On the other hand, suppose that the system was such as the Diesel, where the air temperature itself is to serve as the igniter. In that case, to insure ignition, we should have a margin of about 200° in the other direction; that is to say, the air should be compressed to a sufficiently high pressure not only to produce ignition temperature, but 200° more, in which case the last two columns give the compressions that are necessary. Should the oil now be introduced into the cylinder by means of compressed air to make a fine spray and scatter it through the charge of air, in accordance with the general practice, then it must be remembered that this air jet which is doing the spraying and the scattering is itself exerting a cooling action. Consider 1,000 or 1,200 pounds air expanding through the spray valve into a cylinder with about 300 or 400 or 500 pounds compression pressure, and it can readily be seen that there is a very considerable cooling action right at the jet. That it will act to prevent ignition, and, to make sure that the oil will ignite in spite of such cooling influence at the point of oil injection, it is necessary to carry the compression still higher. Let us provide at least 400° over ignition temperature, in which case with an initial temperature of 200° we would require something between 467 and 800 pounds compression. With 600° initial temperature we could secure this 400° above ignition with anywhere between 80 and 100 pounds compression.

These figures should be made the subject of some study and be used in considering the various structural arrangements, because in them will be found a key to the question: Why cannot this type of engine—this arrangement—be efficient? And why is the other arrangement highly efficient? Here also will be found the key to the question of how we should proceed to make the less efficient one more efficient. These three elements stand out:

First, is the fuel in contact with the air during compression, or is it not?

Second, if the oil is in contact with the air during compression, is the region of contact all of low temperature, all of high temperature, or any part of it a high temperature region when the compression starts? The initial temperature has a great effect on the degree of compression needed or permissible.

Third, if the oil is not in contact with the air during compression, but has to be introduced subsequently, how should it be introduced with reference to the igniter? If the air itself is to be the igniter, then a certain high value of the compression must be obtained, or the engine will not run. If the air itself is not to be the igniter, then what kind of an igniter can we provide? We will look into that question.

All the first oil engines to attain commercial success had vaporizers. That is to say, there was some element, the duty of which was to heat the oil alone for later air mixing, or to heat the oil and air together, to produce a vapor air mixture. It matters very little as to what were the details of those arrangements—whether one produced a heavy carbonizing action and another one did not. They are all one as regards this point of controlling importance: The mixture when formed by any such vaporizing system is necessarily a warm or a hot mixture. Just how hot it has to be depends upon the vapor pressure of the particular oil, or rather the heaviest constituent of that particular oil, and I will give some figures to make it clear. A 60° Baumé gasoline vaporizing in air requires a temperature of about 100° to 110°. The ordinary lamp kerosene of 150 flash point vaporizing in air requires a temperature of 250. A navy fuel oil vaporizing in air requires a temperature of 420, and so on. One could go on with any grade of oil or residue; for each there is some temperature at which the vapor of the oil can be produced in the right proportions for combustion and in contact with its air, without residue and dry. The heavier the oil the lower its vapor pressure or that of its heaviest constituent, then the hotter that mixture must be to be a gaseous mixture; and the hotter it is, according to this table, the lower the compression it can stand without self-igniting. Therefore only those fuels that have high vapor pressures or that can make cold gaseous mixtures can be treated with the whole mass under compression. Those oils which are within the heavy class of oils, and have so low a vapor pressure as to require 300° or 400° initial temperature outside the engine plus a couple of hundred degrees rise in temperature coming into the cylinder, can be compressed hardly at all. Therefore such heavy oil engines cannot by any possible conformity with Nature's laws be efficient. It is fundamentally sound that all those schemes for heavy oil engines that involve vaporizers are utterly incapable of producing an efficient engine.

For some reason or other everybody playing with this class of machine in the early days seems to have been absorbed with the idea of cooking the oil; they seemed to feel that it had to be roasted to death, and then wondered why they could not carry any compression, and why the efficiency was so low and the fuel consumption so high. This fundamental impossibility, against which mechanical ingenuity is absolutely helpless, never hit them at all; and that situation has lasted from the early '70's up till today, and people are still inventing schemes of this kind.

It is perfectly clear that to produce a high efficiency, and at the same time a fairly high mean effective pressure in the interest of high power, the first and fundamental requirement is not only high compression, but to keep the oil away from the air until the time comes to burn it. By keeping the oil away from the air during compression any compression you please is yours for the asking. It is purely a matter of mechanical clearance between the piston and cylinder head. Therefore it would seem possible to secure almost any efficiency in such an engine with the delayed introduction of oil under the system which has received the name—and a very good name—of late-injections engines. Late injection is fundamental to efficiency. I will not say there is no limit to the efficiency, but it is far beyond the common old practice when this principle is adhered to.

Now the late-injection principle of working is highly developed mechanically only in Diesel engines, and therein you see there is a little scientific engineering anomaly. The desired and necessary principle of high compression with delayed injection has been developed in its application to the less promising type of cycle—the Diesel; and the more promising type of cycle, the Otto—that which promises a given fuel consumption with half the compression of the Diesel—has been delayed; so that our engines have been highly developed along the less promising line without scarcely any development at all along the more promising line. Do you wonder, then, that with the success we have had with the less promising we may expect and look forward to much greater success with the more promising field when it is vigorously followed up?

We have come now to this point: That after compression we are to introduce the oil, and the oil may be introduced in such a way as to produce an explosion or to burn at substantially constant pressure without explosion. I want to point out to you next the controlling means in use for doing either one of those things or both of them.

First, as to the Diesel: At the time the oil is introduced the temperature of the air is something higher than the ignition temperature—200°, 300°, 400°, or 500° higher than ignition temperature—and so far in all these engines the oil has been introduced and sprayed by the aid of compressed air. How is the feed of oil controlled so that the rate of combustion shall be just enough to prevent the pressure falling along the re-expansion line, but kept substantially constant? That is entirely a matter of spray-valve design. If you set yourself the problem—especially those of you who have not worked on this problem—of designing a scheme to squirt oil into a cylinder against 400 or 500 pounds compression pressure at a graduated rate for 10 per cent to 15 per cent of the stroke, so as to hold the pressure constant, the oil burning as fast as it enters, you will find some little problem on your hands, and I venture to say that eight out of ten men will start with some kind of cam-driven pump. Now, that will not work at all. The thing that is relied upon is the tendency of the oil to stick to a metal plate. To make that clear, consider the ordinary spray Diesel valve with a fairly heavy stem in a casing. Beyond it there is a graduated orifice outlet, which is the spray orifice and is changeable in size to suit the viscosity of the oil. The oil is pumped into the chamber or space between the casing and the valve stem by a pump which has only one duty to perform, and that is, measure the amount, not time or graduate of the injection. That cannot be done—or it never has been done. I do not think it can be done. The oil, in the ordinary course of events, is put into the spray-valve chamber before the injection is wanted, and it will run down to the bottom more or less fast. If the valve be lifted and the oil itself has reached the bottom or valve seat, the first thing to happen after the opening of the valve is a squirt of solid oil with no spraying whatever. In an instant the oil is all out, and, instead of maintaining a graduated combustion for 15 per cent. of the stroke, we have a lump of oil squirted in without spraying or graduation. There will be a certain amount of combustion, but only part will burn; the rest makes a little lump of carbon.

Now, to secure graduation of oil feed, all that has been done is to stick a lot of plates with holes in them in the space between valve stem and casing. These plates have definite thickness spacing and drilling; sometimes they are given special forms and surface finish, each designer adopting an air of great mystery about his arrangement, and claiming absurd superiority over his rivals.

All that these things do in the oil chamber is to provide an extended surface to which the oil naturally tends to stick. It might be filled with shot and it would no doubt work just the same. The oil is pumped into this chamber and when the valve is pulled open the oil tends to travel down, partly by gravity and partly by the surface frictional influence of the air on the oil. The oil, however, is sticking to that labyrinth surface. The oil chamber is nothing but a mechanical labyrinth. That is what it is called for want of a better name. And that is the only means that works as a graduator of oil feed, except one. The other one surrounds the stem valve by a sort of Venturi tube casing, between which and the valve body is found the oil-receiving chamber. There are two holes in the Venturi partition between the oil chamber and steam chamber, one from the Venturi throat to the bottom of the oil, the other imposing the total air pressure on the oil surface. When the valve is open the compressed air flowing through impinges on that hole to the surface of the oil and builds up its total pressure, while at the same time the other hole, being at the throat of the Venturi, is subjected to a lesser pressure by the air velocity head, thus developing lifting tendency on the oil, and we have a regular vacuum flow injector. This action or the former one of the labyrinth forms the basis of all the schemes for graduations of feed of oil from the spray-valve body to the spray-valve nozzle, so that the oil feeds during the required period of time at a more or less steady rate to this nozzle, and it is torn to pieces or sprayed by the velocity of the air issuing from it. It is clear that with

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this arrangement the rate of oil flow, the time of oil feed, and the rate of combustion will vary with the viscosity of the oil.

Should such a valve be adjusted by the spaces and orifice size to give the required feed for a given oil, and then supply an oil of lesser viscosity, or maybe the same oil supplied hotter, or really a less viscous oil, then it will discharge much more rapidly, and instead of a horizontal combustion line there will be a rapid rise of pressure somewhat like a slow explosion. This indicates what is necessary if we desire to make a spray nozzle suitable for producing an explosion. To produce the explosive mixture and secure explosive combustion of oil with the spray nozzle, the first step necessary is to reduce the flow resistance in the body of the nozzle by removing a considerable amount of the labyrinth surfaces or by reducing the viscosity of the oil by heating it. As a result the oil will be discharged much more rapidly, and if enough resistance be removed from such a spray nozzle it would give a substantially instantaneous injection of oil as a liquid spray. If that occurred we would have a more or less finely divided spray scattered through the mass of compressed air, and if the air is not up to ignition temperature, then we would have produced what might be called a liquid explosive mixture, ready for any kind of ignition that is handy. It is a liquid explosive mixture because no attenpt has been made to vaporize the oil. All that has been done is to make really fine fog of liquid drops suspended in the air, scattered through the combustion chamber. To get such an explosive mixture or charge ready for ignition it is necessary that no part of the interior walls or the air itself touched by the oil be up to ignition temperature, because if the air should be up to ignition temperature, as would be the case with the Diesel, then the oil would ignite as it came in, before the oil spray charge was scattered through the air. To obtain the explosion it is necessary to get all the fuel into the cylinder and scattered through the air before it is ignited, just as with delay electric ignition. On the more desirable Otto cycle with its explosion it is necessary to prevent the air reaching ignition temperature during compression; it must be incapable of igniting the oil, and the ignition, by whatever means is relied upon, must itself be delayed long enough to let the whole charge of oil get in the cylinder, because unless it is in there is nothing that can explode.

Now as to igniters: The common thing has been a hot plate; sometimes a bulb of spherical form with a neck; sometimes a hemispherical bulb without any neck, just forming a cylinder head; and sometimes all sorts of weird curves and twists and shapes—every one of them the subject of a patent. Now, of that collection, what is good and what is bad? There is a simple way of sizing it up. The hot metal—that is, the igniter—must be so related in position to the injection point that no part of the oil reaches the hot plate before all of it is in the cylinder, and the shape such that no part of the oil reaches the hot plate before any other part.

Let me illustrate and you will see the force of this principle. Consider a cylinder with a spray valve at one edge of a nice hemispherical plate, red hot, the oil being sprayed at an angle toward the plate. There have been many made like that. The hot plate comes up quite close to the injector, which discharged a more or less conical spray. Some of the oil will strike the hot plate before the oil is all in the cylinder and considerable of the air. The first oil that hits that plate is going to fire the charge, or so much as has entered, and much of the air is utterly inactive and cannot do anything, as it has received no oil. At the same time, an explosion of part of the charge has taken place and the rest is still coming in. What does it meet on the way in? A red-hot—yes, a white-hot—mass of inert gas. What will happen? Nothing but crack, liberating free carbon and carbon monoxide. The unused and unmixed air leaves free oxygen in the exhaust, and with it the unburned fuel appears as carbon monoxide and much free carbon with plenty of solid carbon cake inside. A very low mean effective pressure and low efficency will result, and the engine will be very dirty.

The ideal condition is that in which no part of the oil strikes the igniter sooner than any other part and none of the oil strikes the hot plate until all the oil is in. Now, what kind of arrangement would give that? Suppose we had a spray valve making a cone-like spray discharging axially into a hemispherical head, the center of which is the spray orifice. Then at the time the oil at full load had reached this hot ignition plate the last oil would have come in if the dimensions were right. That would be in conformity with the principle laid down. That arrangement will give anywhere from 50 to 100 per cent more mean effective pressure than the non-symmetrical arrangement previously described, and with it any compression you please may be carried. It does not matter how hot that latter plate gets, where the former has to be run cool on the side where the oil first strikes and yet hot enough elsewhere not to miss fire. At full load it tends to get too hot and pre-ignite and knock its head off; with a light load it fails to get hot and fails to fire at all. And so the engines in that class are always provided with some kind of scheme to prevent getting too hot at heavy load and too cold at low load. The popular water-injection scheme usually does nothing but wash the lubricating oil off the cylinder wall.

For a long time, practically throughout the whole history of this art, this condition of necessary relation between the spray valve and the hot-plate igniter has not been realized, and yet hot-plate igniters have been in almost universal use. Now hot-plate igniters themselves are a bar to progress, because if we use a hot-plate igniter it has to be an outer wall. It has to be an outer wall so that you can start it with a torch and still not burn up. An inner wall cannot be started. A hot plate submerged in a cylinder cannot be started, because you cannot get a torch at it from the outside. If a hot-tube igniter be provided for starting and an internal plate relied upon for running, as is sometimes done, then you have no control over that hot plate at all and it will burn out. So a hot-plate igniter has to be an outer wall to be practical, and when you have made it an outer wall to be practical, then it is not really practical, because it limits the size. That red-hot plate lacks tensile strength. To be sure, a selected grade of cast-iron can be found that will not lose too much tensile strength, but nevertheless it is very weak in proportion to what it might be. As cylinder sizes go up—and I have already indicated that one of the great fields of need for the heavy oil engine is ship work, which calls for cylinders to go up just as high as we can make them go—then the external hot plate is barred. It is not good because it is too weak structurally. Here is the place, then, where the functional problem leads to a structural debarment and where progress into the larger sizes requires an abandoment of the red-hot plate as an igniter—or at least a very material modification. If we can get along without it at all, then we need not fool with modifications, and as a result of that thought many people have tried various other schemes, and there is now being attained some success with electric igniters and with moderately warmed internal hot plates, which are more properly called "warm" plates. Suppose, with reference to the latter, that we have a 300° internal temperature and one would stop just short of ignition. Suppose we had a 300° intitial temperature; then for practical reasons about 279 pounds compression (see table) would carry the entire air above ignition temperature 200°, and that would not be practical with late injection. If, however, the initial temperature of the air were lower, or a sufficiently lower compression were selected, we could come within 100° or 200° of ignition temperature and still have a very substantial compression. As a matter of fact, it is perfectly feasible to use compressions up to 175, approaching 200 pounds—possibly more—without reaching ignition temperature, provided the initial air is not too warm. Suppose we were injecting straight toward the piston and we had a spot of metal on there that did not get red hot, but raised the temperature of the air right next to it 200° or 300° higher than elsewhere. That is all that is necessary for ignition, and that is one way the thing might be done. A cold external wall for strength, a warm internal spot at the maximum distance from the igniter, not red hot but sufficiently warm to supply a couple of hundred degrees difference between the temperature attained by compression (which is not high enough to produce ignition) and the amount necessary to actually produce ignition at that one spot alone.

We do not know much as yet about that scheme, except it is possible to run that way. Whether it can be developed into a practical controllable thing remains to be seen. I am inclined to think it can. If it can, then one serious bar to the increase in size and more extended use of the Otto cycle engine has been removed and a new field of most interesting and valuable possibilities opened up.

Without that, however, there is still another way of proceeding that is receiving some attention. For a long time back it has been known that with a sufficiently fine and proper kind of spray of oil into air an ordinary spark plug would serve as an igniter. With the wrong kind of spray and plug the oil would collect at the spark plug and short-circuit it, but a spark plug can be made to ignite the right kind of spray just as positively as it ignites a gaseous mixture. An apparatus involving the lighting of sprays by spark plugs is going to receive considerable impetus in the near future.

As to engine schemes: Suppose we had a thoroughly cold interior engine—an engine the walls of which were all cold, just as cold as we can get them, and to my mind you cannot get them cold enough for a good practical oil engine—the colder the better. Into that cold cylinder we admit a charge of air and compress it, but not sufficiently to produce ignition, and when we get a sufficient compression to produce a highly economical engine as to oil and sufficient to produce a mean effective pressure approaching 100 pounds per square inch, then ignite with this spark plug. That can be done, and it is a most interesting possibility. It is particularly interesting because in conjunction with it it is possible to use a modified oil spray.

With the standard Diesel engine the oil-spray valve is supplied with compressed air under a thousand pounds and more. Now, in the actual running of these engines the air compressor to supply that air and the air storage are just about as big a nuisance as the engine—sometimes, in fact, and often, more. If we could get rid of that, it would be a splendid thing to do. A "solid" spray is therefore a highly desirable thing.

So far we have succeeded in making a number of engine solid-spray valves that work quite well, and they work so well that I feel confident that the end of the compressed-air spray is drawing near, and that thereby we are going to eliminate another big source of trouble. I am not sure that the solid injection spray I have in mind will work on the Diesel engine, because the Diesel engine requires a graduated feed, and I am not at all sure that the principle of action of the solid spray can be adapted to the graduated feed. It works splendidly with the instantaneous feed for the Otto cycle or explosion engine. There is only one principle that need be kept in mind to explain the many ways of working out the solid spray, and that principle is pretty old. It is the principle of the navy fuel oil-burning spray for boilers. The navy and other ships cannot afford to use compressed air or steam for spraying oil, as is so common on land, because they have not the fresh water with which to produce this steam. They are therefore compelled to use a mechanical spray, and this type of spray has been very highly developed. It involves nothing more than a minute orifice from which the oil is discharged with a combined motion axially through the hole and rotary as it comes out, the rotary motion being developed before the oil reaches the hole in a rifling tube or tangential feed passage to a little whirl-chamber just back of the spray orifice. By giving the oil a whirl behind the spray orifice with a proper exit from the orifice it issues with a motion in two directions—axial and rotary—producing a sort of solid conical spray that is fine and works well in an engine. By making the passages small enough in such a spray valve so that they are practically capillary, then a direct-acting pump plunger slamming against the solid column of oil will make such a spray without any dribble at the ends of the spray period. Remember that no dribble at the end or beginning of a spray is permissible, because every drop that dribbles carbonizes. To avoid the dribble the only thing necessary is to get the passages substantially capillary in size, and that is not difficult. At first glance it would seem as if that boiler spray valve could not be made to work with rapid intermittence, but it can and is so working today.

Now a word or two on the structural side. We may assume that this functional question has in it considerable undeveloped possibilities, and that these are not mysteries, but are lines of purely rational progress, the key to which we have in every instance, and nothing is unknown except how far you can go. We know the route every time; but we do not know how far we can go. Assuming that you can see with me in your mind's eye non-Diesel, late-injection, high-efficiency oil engines working as well as the Diesel—or better—then arises the question, How should the structure be built? Are there any lines of standardization that can be well and properly applied there? Or is every designer to regard himself as the one selected by the Almighty to produce something that no one else ever made? For some strange reason, that seems to be in the minds of almost all oil engine designers. While for years we have been building shafts, frames, and engine structures for steam engines and similar structures for pumps and compressors, just as soon as an oil engine designer gets his hand over a drawing-board he wants to throw all that experience away and get up some other thing and make more trouble than do the parts that are peculiar to the oil engine.

The point of view to take with regard to this structure is this: There is no essential difference in kind, except the cooling problem, between an oil-engine cylinder and a steam-engine cylinder. The difference is one of degree in the structural problem. There is no essential difference, certainly none in kind, between the problem of the frame and the bed plate and the rest of the running gear of the oil as compared with the steam engine. That being the case, these queer arrangements that have crept into the oil-engine field and that have become more or less standard there, quite contradicting all previous experience and established custom with the more firmly established steam engine, cannot be accepted. There is no difference whatever in the design of a column to support a bridge, a pier, or a building, or to support a statue; all that counts is the load to be placed on it. And yet the oil man has persisted in carrying out his structure from

cylinder down to bed plate on radically different lines than have long been standard with steam. The result is hopelessly bad. Let us bring out some of our good old designers who understand the designing and building of structures to carry loads, and I would not tell them anything about the oil-engine peculiarities at all. Some other man is taking care of that. Then we will get somewhere.

Just to point out one or two things, to illustrate what I have in mind, consider the most widely used Diesel cast-frame structure extending from bed plate to cylinder head and being an axially expanding cylinder line. This cylinder line is a stressed member, the stress being bursting stress, with 400 or 500 pounds per square inch normal pressure at the top, and possibly an accidental maximum pressure with a pre-ignition pressure of twice that amount —1,000 or 1,200 pounds per square inch. Now we certainly know how to build cylinders to withstand bursting, so this is not peculiarly an oil-engine problem, except that its expansion must not be restrained. That cylinder structure is inside of a heavy casting and held at the top. This casting is practically an "A" frame with a cylindrical extension at the top; the cylindrical extension forms the water-jacket and is faced at the top on which the cylinder line flange rests. There is a slip joint at the other end of the line to allow of longtiudinal expansion of the cylinder line barrel with reference to the jacketed frame. An engine built along such lines as that is fundamentally wrong, because the jacket is at the same time a frame. What is its principal duty? Its principal duty is to hold water—and anything that will hold water will do just as well—and yet you will find big engines with jacket walls of 1¼-inch cast-iron for many feet in diameter. Think of all the weight that goes with that, and for what? To hold water and transmit an upward tension load from the cylinder head down to the main bearing. Nothing could be more fundamentally wrong. In the first place, as to the material: It is all cast-iron and is always in tension if it is loaded at all. If it were a steam engine there would be an alternate compression and tension; but here it is tension loaded, and if there is any worse material than cast-iron for carrying a tension load, it is probably cast aluminum. Furthermore, if such a cylinder line barrel is strong enough to resist bursting, it has just twice the factor of safety for the longitudinal tension stress, which means that the double metal of line and jacket is entirely unjustified. The cylinder should be held to the frame at its crank end and the jacket eliminated as a stressed member. There is no reason for carrying a heavy cast-iron wall for a jacket, when ordinary tank steel will do the job just as well and a little bit better. We have oxygen welding apparatus that will enable us to weld the edge of a thin sheet to cast-iron with perfect success and with not much expense.

There is then a necessity for changing the cylinder to take off that unnecessary weight. Do you know that some of these engines of stationary form—the class I am condemning—weigh 600 pounds per horse-power? Think of it! And when these same engines are refined down for ship work without changing the type of structure, just the same typical arrangement, but reducing the bed plate and getting rid of fly-wheels, they come down to about 150 pounds per horse-power. But there they stick. To go below that it is necessary to change the structural arrangement to conform more and more nearly to the better and more refined class of standard steam practice, and in this country there are excellent models for them, and all that has to be done is to follow them out and keep within the same method of calculating loads and factors of safety—keep the same factors and the resulting work. just as well as in the steel. That means not only the taking away of this cast jacket and making the cylinder barrel carry the two kinds of stress—the bursting and the longitudinal at the same time, which it is perfectly able to do—but it means also to take this heavy cast-iron frame structure away from the space between the cylinder end and the bed plate and to substitute for it the steel tension rod construction of the torpedo boat or yacht engine—to mention the two most highly developed forms of that structure. To be sure, that type of engine is not a stiff engine. Certainly the cylinders are going to sway and weave. But what of that? Where did these oil-engine designers get the idea that this thing had to be a stiff structure, like a brick wall? I do not know where, but they got it, and there it sticks.

The most successful engines we have today, and the most wonderful machines that we have ever designed, are steam locomotives. When you consider what they have to do and how well they do it, they are truly wonderful. What would happen to a locomotive that was built stiff? And what would happen to the rails on which it runs? You would have neither locomotive nor rails at the end of the first run. The steam engine that gives years and years of service and is regarded a a model of reliability, carrying a ship trip after trip, back and forth across the ocean like a ferryboat—is that a stiff structure? It is not; it weaves and twists, but that does not hurt it. Why, then, should we insist upon the cast-iron stiff structure for oil engines? There is no reason on earth why we should. So I say that we can take the steam-engine structure from bed plate to cylinder and design and apply it to oil engines, selecting dimensions to give similar elastic factors, bearing loads, and so forth, and it should give equal success.

After these two changes it will be time to remove the outside cast wall of the cylinder head. By taking the cast head jacket away from the top of the head we will free the valve housings, the intake and exhaust ports, the housing for spray valve and starting valve. These will then be exposed and the present elaborately cored casting eliminated. These cored heads are so awful the foundrymen and molders are not to be blamed for doing with them what they always do when they get into a corner with cores that are too intricate—they knock a piece off; and then we wonder why the head overheats. The designer thought he had provided for water, but the molder could not get it there, and the designer is responsible for asking a molder to do what no man could do. In proportion as that casting is made easy to mold, so it will be a good job. By means of the oxygen weld the whole cylinder head top can be left off and the only cores left are the cores for the intake and exhaust, which are easy cores. Then you get a clean casting which can be inspected on both sodes and free of bad shrinkage stresses, add a sheet jacket welded around the edges, and you will have a real job.

Just as an indication of what happens when this thing is carried out, even in part, I will tell you that the most highly-developed of these oil engines today are the submarine engines. In those enginese the heavy cast frame structure in the better of them has been removed, and, as a consequence of the steam-engine practice as to frame structure, those engines which in the older form of oil-engine structure weighed 150 pounds, more or less, to the horse-power have come down to what? Thirty-five or forty pounds per horse-power. They still, however, are carrying the cast-iron jacket and a cast-iron roof to the head, so there is room for saving some more without doing any harm whatever.

To conclude, then, with a word or two about the auxiliaries—especially with ships, because you may get a perfectly fine engine, and by wrong kinds of auxiliaries or wrong arrangement of auxiliaries have an awful hard engine-room to work. The tendency for some time has been to regard the engine structure, when it comes to auxiliaries, as a sort of Christmas tree on which you hang anything you want to use. If they decided to put a fire-alarm in the engine-room, I have no doubt that they would stick it on the engine somewhere. You find circulating pumps, air compressors, water pumps for circulating water, and oil pumps, in some cases bilge pumps—all hooked on to the main structure of the engine, which, in some cases and quite a number, is 2-cycle, with a row of scavenging cylinders stuck along one side, so that they resemble the old river boat engines. It certainly makes a weird combination, and in the old-day steam-engine practice just that thing was done. They carried feed pumps on the engine, condensers were mounted on the engine, and hot well, and circulating pumps likewise. It took us 150 years to get rid of that arrangement, and it is no longer standard practice. The engine structure should be preserved and respected as an engine structure, and not interfered with it all by hanging any kind of trimmings on it that have the slightest possibility of interfering with access to the engine. Doubly so is that true when the auxiliary attached to the engine does not and cannot behave right as attached.

In the latter connection I would call your attention particularly to the circulating pump for water. When the circulating pump for water is driven off the engine, either by its own crank or by rocker and links from other moving parts, then when the engine stops the water stops, and when the engine starts the water starts. That might seem to be a desirable thing at first glance. As a matter of fact, it is positively dangerous, and many cracked cylinders and piston heads can be traced to that cause alone; because, consider the interior of a cylinder when the engine is running at full power, and you will see that there are some hundred pounds of metal very hot—quite hot in proportion as it is thick. Now when that engine is shut down heat is there as insensible heat of the metal, and the jackets are full of water—X pounds of water and Y pounds of hot metal. If the weights and heats involved are figured out you will find in a big engine there is enough heat in the metal to evaporate all the water to steam and you would still have some left to conduct out to more distant parts, which should be cold—parts which are designed to be cold, but which, when hot, crack. Especially is that true when after such a shutdown and a general heating somebody starts the engine and shoots a lot of cold water up against the surface. That condition can be eliminated only by completely independent jacket water circulators separately driven by another engine, which permit the running of the water so long as water comes out from the discharge with any rise in temperature over the intake. In other words, the water should be continued through such an engine until it ceases to become warm, and that may be from twenty minutes to half an hour after the shutdown, depending on the size and style of machine.

A similar discussion could be carried on about other auxiliaries. This question of auxiliary arrangement and starting by an auxiliary engine is just as important for study as the engine itself. There is just as much room for improvement as in the engine structure, as a structure, or the engine parts that are concerned with its functional operation.

Getting back now to the beginning, it must be admitted, and the more you study it the more willing you will be to admit it, that, while we have made very considerable progress with the heavy oil engine, that progress is by no means ended. In fact, we can go much further yet than we have already gone, and we can make the heavy oil engine meet conditions it has heretofore failed to meet.

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